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PHYSICAL REVIEW.

THE ATTENUATION OF SOUND.

By A. Wilmer Duff.

In an earlier paper[1] the author stated the results of an experimental method for finding the rate of decay of the intensity of sound with distance. Prefixed to this statement of experimental results was a mathematical investigation, founded on methods developed by Stokes, Kirchoff and Rayleigh, of the parts played by viscosity and conduction and radiation of heat in the attenuation of sound. The general result of this discussion will be here restated and amplified with a view to facilitating the understanding of a second experimental method, the results of which it is the chief purpose of the present paper to describe.

It was found that the radial velocity of a gaseous medium through which spherical waves are passing is proportional to

$$e^{-mr}\left\{\frac{n}{ar}\sin n\left(t-\frac{r}{a}\right)-\frac{1+mr}{r^2}\cos n\left(t-\frac{r}{a}\right)\right\} \qquad (1)$$

in which n denotes the vibration frequency $\times 2\pi$, a denotes the velocity of sound, r denotes the distance from the source, and m consists of the sum of three terms depending on viscosity, conduction and radiation respectively; namely

$$m=\tfrac{2}{3}\frac{n^2}{a^3}\mu'+\frac{\gamma-1}{\gamma}\frac{n^2}{2a^3}\nu+\frac{\gamma-1}{\gamma}\frac{1}{2a}\lambda \qquad (2)^2$$

[1] Phys. Rev., 6, p. 129, 1898.

[2] This expression was incorrectly stated in the original paper, but the error does not affect the subsequent calculations which were made from the correct expression as here stated.

μ' denoting the kinematic coefficient of viscosity of the medium, ν denoting the conductivity of the medium, λ denoting the constant of radiation according to Newton's law, and γ, the ratio of the specific heat at constant pressure to that at constant volume.

From (1) it is seen that the motion at any point may be regarded as the resultant of two simple harmonic motions, differing in phase by a quarter of a period. The relative importance of these terms differs widely for different values of r. For large values of r the second component is negligible, since m is certainly a very small quantity, and the motion is represented by

$$\frac{e^{-mr}}{r}\sin n\left(t-\frac{r}{a}\right). \tag{3}$$

At small distances the term mr may be neglected in comparison with unity and the resultant motion is represented by

$$\frac{e^{-mr}}{r}\sqrt{1+\frac{a^2}{n^2r^2}}\ \sin\left\{n\left(t-\frac{r}{a}\right)-\varphi\right\} \tag{4}$$

in which $$\varphi=\tan^{-1}\frac{a}{nr}.$$

Moreover, for small values of r, e^{-mr} does not differ appreciably from unity. Hence the law of intensity at small distances is

$$\frac{1}{r^2}\left(1+\frac{a^2}{n^2r^2}\right) \tag{5}$$

while that at large distances is

$$\frac{e^{-2mr}}{r^2}. \tag{6}$$

The method described in the previous paper consisted in observing the distance to which eight similar small whistles blown under a definite pressure were audible and also the smaller distances to which the whistles arranged in pairs were audible. Five different sets of such observations, made under widely different atmospheric conditions (except that in all cases there was no wind), gave consistent results, the mean value of m being 0.000042 per cm. Subtracting from this value of m the parts due theoretically to viscosity

and conduction, there remained 0.000035 due to radiation and other causes not taken account of in the mathematical discussion. Of such unconsidered causes the only ones that readily suggested themselves were refraction and internal reflection, and it did not seem probable, in view of the close agreement of results obtained under widely different atmospheric conditions and other circumstances, that these could have largely affected the final result. Hence a value or rather an upper limit of 8.3 was deduced for the hitherto unknown constant of radiation of air in Newton's law of radiation.

In a discussion [1] of the preceding results which Lord Rayleigh has contributed to the *Philosophical Magazine* the ground is taken that the upper limit thus assigned to the constant of radiation must be very much higher than its actual value (so much higher in fact that only an entirely inappreciable part of the rate of decay actually observed could have been due to radiation) and that, if no part of the decay of intensity can be shown by further observations to be due to atmospheric refraction, a further cause must be sought. This cause Lord Rayleigh thinks may be found in the time required for the kinetic energy produced by compression, which at first is purely translational, to become shared between the translational and rotational forms. The suggestion thus made seems to be of such great importance that it would be well if any evidence bearing on it could be obtained.

In the meantime it seemed well to attempt by an independent method to settle whether the rate of decay of intensity is really as high as the previous experiments seemed to show. The results obtained by this second method are still somewhat imperfect, the work having been discontinued owing to unfavorable weather; but, as an opportunity for repeating and extending the work may not present itself for a considerable length of time, it has been thought permissible to publish the results already attained.

The sounds studied by this second method were produced as before by whistles blown under definite pressure, the observations being made at a very quiet part of the River St. John in New Brunswick. To obtain a variable standard with which to compare the sound at different distances, a telephone transmitter was placed near

[1] Phil. Mag., XLVII., 308, March, 1899.

the whistle and a receiver, connected to the transmitter through a line of constant resistance, was held so that its distance could be varied until the intensity of the sound heard directly was equal to that of the sound given by the receiver. At first only a single whistle was employed, it being placed in a heavily padded box that could be alternately opened and closed, the telephone circuit being closed when the box was closed and opened when the box was opened, so that the sounds to be compared were heard alternately. Afterwards it was found better to employ two whistles of the same form and tuned by adjustable plugs to the same pitch. One, mounted in the open air, acted as the source of sound to be studied, the other was enclosed in the box with the transmitter and the two were blown alternately from the same wind chest. The whistles were sounded by an assistant on an isolated pier in the middle of the river at an elevation of about eight meters above the surface of the water. The observer stood in a canoe which was allowed to float down stream from the pier and arrested at any desired point by means of a cord attached to the pier, this cord also affording a means of determining the distance from the pier. The telephone line consisted of 300 meters of twin wire doubly insulated with rubber to protect it from the water, the whole line being always in circuit. Only one ear was used in making the observations, the other being filled with wool and closely covered by a heavy woolen pad. The distance of the receiver from the ear was taken as the distance from the disc to the outline of the skull adjacent to the ear.

In adopting this method it was hoped that the quality of the direct sound and that of the sound from the telephone receiver would not be found to differ appreciably for the high pure tone employed; but in making the observations the ear became more and more sensitive to minute differences of quality between the sounds compared. These differences seemed to vary with the pitch of the sound and the position of the transmitter relatively to the whistle that acted on it. For only one particular pitch did the differences seem to disappear entirely, namely for a pitch of about 4,000. For this particular note, the sound heard directly and that emanating from the receiver were at times so nearly identical as to be entirely indistinguishable when the receiver was held at the proper distance.

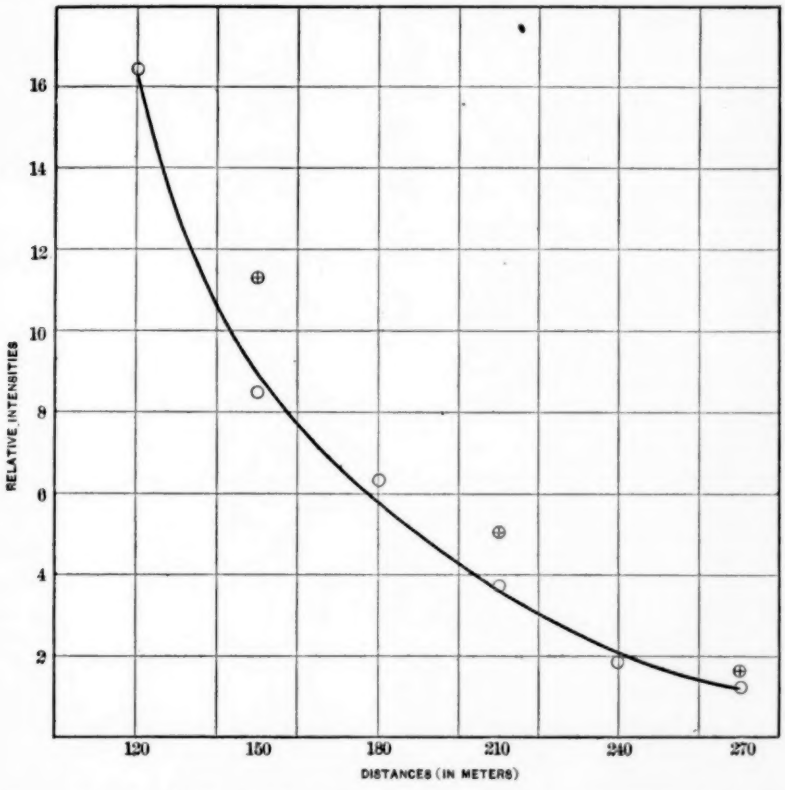
It may be here noted, as a matter of independent interest, that this frequency of 4,000 was afterwards found by calculation to be also that of the free vibration of the iron disc of receiver, not the fundamental vibration, however, but those of the first higher mode. Unfortunately the point was not foreseen and the range of the sounds tried probably did not include that corresponding to the fundamental vibrations of the disc. It seems probable that a telephone (with a carbon transmitter and an iron disc receiver) transmits comparatively unmodified only those notes which synchronize with one of the modes of vibration of the disc of the receiver. I am not aware that the point has been carefully studied.

Several preliminary trials showed the disappointing fact that the ear is curiously incapable of distinguishing small differences of intensity of sounds of appreciably the same pitch and quality. The weakness of the ear in this respect is in marked contrast with its power of discriminating minute shades of pitch and quality (a fact that seems capable of being readily accounted for on the theory of evolution of organs by their usefulness) and in great contrast with the power of the eye in judging equality of illumination in the ordinary photometric method. An attempt to improve the ear in this respect by some practice (and it may be stated that the writer's ear is a fairly good one in the ordinary musical sense) seemed to render it still more acute to minute differences of quality. Hence the only hope of eliminating errors lay in multiplying the observations at each distance. This will explain the numerous readings in the table of observations, made between 10 a. m. and 1 p. m. of an exceptionally clear, quiet day with few clouds, the average temperature being 30°C.

In the table the observations are entered as made, with one exception. The last three readings at 120 m. were made on returning to that distance after the readings at 180 m. and 240 m. had been made and similarly as regards the last four at 150 m. The purpose was to ascertain whether atmospheric conditions had appreciably changed. No change was detected in the first case; in the second there seemed to be a slight change, but the evidence was uncertain. In one unfortunate respect there was a break in the continuity of the readings that is not indicated in the table. Toward the end of the

Distance from source (in meters).	Setting of receiver (in cm.).	Mean Setting.	Relative Intensity.	Intensity × (distance)^2 ÷ 10^4
	25.0			
	23.8			
	26.3			
120	23.7	24.7	16.4	23.6
	23.8			
	24.5			
	26.2			
	37.5			
	53.7			
180	41.3	40.7	6.33	20.5
	36.9			
	34.3			
	71.3			
	78.7			
	71.2			
240	76.3	74.2	1.84	10.6
	71.3			
	76.2			
	36.2			
	28.8			
	26.7			
	31.3			
	32.5			
150	26.2	29.8	11.3	25.4
	30.0			
	27.5			
	31.2			
	27.5			
	55.0			
	40.0			
	45.0			
210	50.2	44.8	4.98	21.9
	39.5			
	38.8			
	45.0			
	92.5			
	87.5			
	77.5			
	70.3			
270	83.7	78.6	1.62	11.8
	71.3			
	66.5			
	94.2			
	70.0			
	72.5			

first half of the series of readings the ear became sensitive to a slight difference of quality between the direct sound and the telephonic sound. To remedy this, the transmitter was moved slightly further away from the whistle. This, of course, made the instrument less sensitive, and raised the succeeding estimates of the intensity of the direct sound in a constant ratio as compared with the earlier ones. This is shown in the accompanying plot of the results, in which the



crossed circles indicate the intensities at 150 m., 210 m. and 270 m. given in the table, while the plain circles immediately below them indicate the values they would have if multiplied by the reducing factor 0.75. With this reduction all the results seem fairly well represented by the curve drawn, the divergence being not greater than would naturally be expected from the difficulties in the method already referred to.

If now we calculate the mean value of m from each set of obser-

vations it turns out to be .000034 in the first case, and .000032 in in the second case If we reduce the second set in the constant ratio suggested, and then use the whole, with the mean value of 0.000033 for m, for testing the theoretical law, we get the following table:

Distances (in meters).	Observed intensity.	Intensity from curve.	Intensity by Theoretical Law.
120	16.4	16.4	17.9
150	8.5	9.0	9.1
180	6.3	5.8	5.4
210	3.7	3.6	3.2
240	1.8	2.1	2.0
270	1.2	1.2	1.3

A comparison of the last column with the two preceding shows that while the agreement is not wholly satisfactory the differences are not greater than might be expected to result from the difficulties and uncertainties inherent in the method. The range of intensity covered by the above test is considerable—from 16.4 to 1.2. The evidence on the whole is strongly in favor of the theoretical law.

The mean value of m given by the preceding is 0.000033. If we subtract the part of m due to viscosity and conduction, as calculated from the first two terms of formula (2), we get 0.000031 as the part due to radiation, which is seen by (2) to be independent of pitch, and to any other causes not included in the mathematical discussion. If this result be compared with the earlier one obtained by a wholly different method, viz: 0.000035, it will be seen that the agreement is very satisfactory. The difference is not greater than might be expected from the admissible errors in either or both of the methods. The agreement seems at least to fix the order of the quantity in question with a fair degree of reliability.

Now, according to Lord Rayleigh's conclusion as to the possible magnitude of the constant of radiation, the above rate of decay (subtraction having been made of the parts due theoretically to viscosity and conduction) is hundreds of times greater than can be attributed to radiation. It remains to be considered whether it can be due to refraction. In this connection it is to be noted that, while the whistle and ear were respectively 8 meters and 2 meters above the level of the water, the acoustic ray from the whistle to the ear, when the latter was at its greatest distance, would not, under normal

atmospheric conditions, fall at any point more than one fifth of a meter below the direct line ;[1] and, for atmospheric conditions differing from the normal, the depression would be proportional to the gradient of temperature. Hence an appreciable effect due to refraction seems very improbable. Moreover it should be noted that the elevation of the whistle in these later experiments was from three to eight times as great as in the earlier experiments, and in the latter it varied considerably on the five different occasions of observation, while the temperature gradient must have varied widely. Hence the agreement of the earlier results among themselves and the agreement of their mean with the later results would both seem inexplicable were refraction an important cause of the decay observed. It would seem to the writer more probable that, while, under ordinary circumstances of observation, refraction may play an important, perhaps a dominating part, under the exceptional circumstances here selected its effects were comparatively slight.

For reasons stated in the earlier paper, it is very improbable that internal reflection played an important part. Thus the evidence points strongly to the existence of some additional cause of decay of intensity, one whose effects are in accordance with an exponential law of decay.

Law of Intensity at Small Distances.[2]

In the preceding it has been assumed that the intensity of a sound, whose vibration frequency is 4,000, emanating from a telephone receiver, varies, at distances of 20 to 80 cm., according to the theoretical law (5), which, for these distances, does not differ appreciably from the law of the inverse square of the distance. Of this it does not seem possible, (the dissipation at such distances being negligible), that there can be much doubt. Nevertheless an experimental test seemed of interest.

The intrinsic intensity of a whistle (vibration frequency = 4,000) was varied by different coverings and its intensity at a convenient distance was estimated (1) by a telephone receiver as explained

[1] Rayleigh's Theory of Sound, II., § 288 (6).

[2] In a résumé of this work printed in the Proceedings of the A. A. A. S. (1899), it was stated that the results of the following method were so inconsistent that nothing positive could be deduced, but a revision of the calculations has shown the presence of a continued error.

above and (2) by the same receiver shunted by a wire of suitable resistance. The ratio of the estimates obtained by the two series of comparison should be a constant, provided that the true law of intensity at small distances has been employed.

In the following table the first column contains the mean distances of the receiver with no shunt, the second those with the shunt, the third the ratio of the intensities according to the law of inverse squares, and the fourth the ratio according to the theoretical law (5) stated in the earlier part of this paper.

d.	d'.	Inverse squares.	Theoretical law.
20.7 cm.	8.75 cm.	5.60	5.70
38.7	16.5	5.50	5.52
57.2	23.5	5.92	5.92
108	43.5	6.17	6.17

The last column is as nearly constant as could be expected when the difficulties of such comparisons, referred to earlier, are considered. Moreover, it is to be noted that the range of distance covered by the test is much wider than that employed on the previous test.

HISTORICAL NOTE.

Since the preceding work was completed, I have discovered, in a paper by Wien,[1] a record of some experiments made, with a view to testing the law of inverse squares. The amplitude of the sound at different distances was studied by means of an aneroid barometer arranged for great sensitiveness. According to the law of inverse squares of the distance, the product of amplitude of vibration by distance should be a constant. The following is the most consistent of the three sets of observations made, r being in meters.

r	23.1	33.2	48.1	61.5	83.7	137.8
Ar	485	478	457	436	377	375?

The author states his belief that his observations establish the law of inverse squares and accounts for the diminution of Ar by friction on turf, motion of air and slight unevenness of the ground. Nevertheless it is not quite evident that these causes should be so much more effective at greater than at smaller distances, and it seems probable that a considerable part of the diminution was due to causes similar to those studied in these papers.

POLYTECHNIC INSTITUTE, WORCESTER, MASS., May 1, 1900.

[1] Wied. Ann., XXXVI., 1889.

SOME LECTURE ROOM METHODS IN THE ELEMENTARY THEORY OF ELASTICITY.

W. S. FRANKLIN.

NEXT to thermodynamics the elementary theory of elasticity is perhaps the most difficult branch of physics to present to a class. The importance of this subject to the engineer, however, demands a more or less adequate treatment of it in our technical schools and every possible means should be employed to render the details of the theory concrete and to develop the geometric-mental images which form the basis of any understanding of the subject. The object of this paper is to describe some lecture experiments which have been found by the writer to be helpful in the presentation of this abstruse subject in the lecture room.

The elementary theory of elasticity may be conveniently divided into four parts, (*a*) Strains, (*b*) Stresses, (*c*) Stress-strain relations, (*d*) Particular cases such as the bent beam and twisted rod. Part (*c*) is not satisfactorily treated in any of our text-books, as it seems to the writer, and at the end of this paper an outline is given of what seems to be the most satisfactory elementary development of stress-strain relations.

Concerning strains it seems to the writer to be exceedingly important that the student get a clear idea of the character of homogeneous strain, that he understand the significance of the elements of a homogeneous strain—namely three mutually perpendicular stretches, and that he see the applicability of the notion of homogeneous strain to the small parts of a body non-homogeneously strained. In the attainment of these objects the notion of the strain ellipsoid is of the greatest usefulness. Now most students are not sufficiently familiar with solid geometry to get a clear idea of the strain ellipsoid and the writer finds it helpful to use the distortion of an elastic sheet as an illustration in two dimensions, where the relation of the ellipse to the circle gives the geometrical picture of the

strain. An extremely thin sheet of rubber, slightly oiled with a non-mineral oil to make it semitransparent, is stretched over a light frame fifteen or twenty centimeters square. This state of the sheet is taken as the normal or unstrained state and small circles (also a large circle) are marked upon it. The small circles may be projected by means of a lantern. The sheet may now be stretched homogeneously, the sheet being fastened to the frame by a number of spring clips, and the circles are seen to be changed to similar and similarly placed ellipses. The student's knowledge of the relation of the ellipse to the circle, gotten perhaps from his work in projection drawing, enables him to realize that the distortion of the sheet at each point consists of two mutually perpendicular stretches, while the similarity of the ellipses shows that the strain is homogeneous. Stretching the sheet non-homogeneously the small circles are changed to dissimilar and dissimilarly placed ellipses. The attention of the student may then be directed to the fact that the large circle seen directly differs sensibly from an ellipse while the small circles *even when greatly magnified on the screen* are sensibly elliptical. It is also instructive to show the character of the distortion of square figures marked upon the sheet.

Concerning stresses, it is very important that the student get a clear idea of homogeneous stress, that he understand the significance of the elements of a homogeneous stress, namely, three mutually perpendicular pulls, and that he see the applicability of the notion of homogeneous stress to the small parts of the body, subjected to a non-homogeneous stress. These things cannot, perhaps, be treated experimentally in the lecture room, and, therefore, it seems advisable to return to the experiments with the rubber sheet after a statement of the fundamental fact of the stress-strain relation for isotropic substances, namely, that the axes of stress and of strain are parallel, the student being led to infer the stresses from the observed strains.

In the discussion of shearing strain, the writer uses a large, square, hinged frame, over which muslin cloth is stretched, the threads of the cloth being parallel to the sides of the frame. The reduction of a shearing strain to two diagonal stretches (mutually perpendicular, of course) is most clearly shown by drawing a

large circle upon the undistorted muslin. This circle is changed by the distortion to an ellipse with its axes diagonal to the frame.

In the discussion of the strains (and stresses) in the twisted cylinder, the writer uses the following model: A large tin cylinder is covered with muslin cloth, which is fastened to the cylinder at one end and to a loose circular disk at the other end. Squares and circles marked upon this cloth show very clearly the strains (and stresses) produced when the circular head is slightly turned. In particular, the student can see that the cylindrical shell of muslin is lengthened along a set of helical lines which are inclined at an angle of 45° to the straight elements of the cylindrical surface, and shortened along another set of helical lines, which are everywhere at right angles to the first set. The student can infer the existence of tension along one set of helical lines, and the existence of compression along the other set of helices, and he can infer from a previous discussion of shear that tangential stresses act across all horizontal lines and across all vertical lines.

Stress-strain Relations.

Considerations of symmetry show that axes of stress and axes of strain coincide in isotropic substances. This is the first fact to be brought out in the discussion of stress-strain relations. Further development should, it seems, be based upon the experimental study of longitudinal stress and the strain produced thereby. Let P be the longitudinal pull, and let p', q' and r' be the corresponding stretches.

Experiment shows that when p' is small, it is proportional to P or equal to aP.

Also experiment shows that q' and r' are equal to each other and negative (*i. e.*, contractions), and that they are proportional to P or equal to bP. Therefore

$$\begin{aligned} p' &= aP \\ q' &= -bP \\ r' &= -bP \end{aligned} \qquad (1)$$

Similarly, a pull Q (parallel to the stretch q' above) produces the stretches:

$$\begin{aligned} p'' &= -bQ \\ q'' &= aQ \\ r'' &= -bQ \end{aligned} \tag{2}$$

And a pull R (parallel to the stretches r' and r'' above) produces the stretches:

$$\begin{aligned} p''' &= -bR \\ q''' &= -bR \\ r''' &= aR \end{aligned} \tag{3}$$

Experiment shows that the stretches (small) produced by a number of pulls acting together are equal to the sums of the respective stretches produced by the pulls acting separately so that the stretches P, Q and R acting together produce the strecthes:

$$\begin{aligned} p &= aP - bQ - bR \\ q &= -bP + aQ - bR \\ r &= -bP - bQ + aR \end{aligned} \tag{4}$$

The equations express the stress-strain relation for isotropic substances. They are based upon a semi-axiomatic fact and three experimental facts. The first experimental fact may be readily demonstrated experimentally, and the second may be illustrated by a rubber band, but the third is scarcely within reach of the hasty experimental methods of the lecture room.

SOUTH BETHLEHEM, March 13, 1900.

SPARK-LENGTH AS MODIFIED BY SOLID DIELECTRICS.

By W. J. Humphreys.

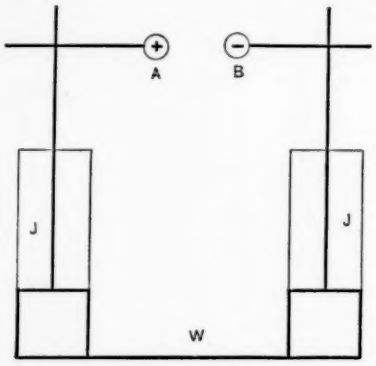
WHILE trying some experiments with an electrical influence machine I noticed that the spark-length was greatly increased by the presence of solid dielectrics, such as glass or ebonite, at certain points near the positive pole. The phenomenon was entirely new to me, and as I was unable to find it described, or even mentioned, in either those books or journals to which one would go for such information I decided to publish an account of my observations.

After my experiments were completed my attention was kindly directed to the fact that Messrs. Queen & Co., of Philadelphia, Pa., furnished an electrical influence machine with radically different poles—one being a metallic sphere while the other is a metal plate with an ebonite disk at its center. I have also learned that Professor Anthony once used a somewhat similar machine that was imported from Berlin about 1870. It seems, therefore, that the action of solid dielectrics on the spark-length has been known for a long while, at least to certain instrument makers, and I venture to publish the following account of my own experiments along this line because I have been unable, after considerable search and numerous inquiries, to find any reference in scientific literature to this interesting phenomenon.

During the course of the experiments several machines were tried and with equal success, but the one commonly used was a two-plate Winshurst machine, the plates being twenty inches in diameter, of ebonite and furnished with metallic sectors.

The accompanying sketch shows an arrangement that gives excellent results, though, as will appear from the experiments, it may be greatly modified and the phenomenon still obtained.

J, J are two small leyden jars, *W* a wire joining their outer coatings, *A* and *B* discharging balls connected one to the positive the other to the negative side of an influence machine, or other source of high potential, and also to the inner coating of its respective jar.



Let the inner coatings of the jars be oppositely charged, as indicated, and the distance between *A* and *B* gradually increased till sparks cease to pass. If now the potential difference be maintained and a glass rod say be placed quite near that side of the positive pole that faces the negative, the air will in general break down and allow a full noisy spark to pass. Nothing of the kind happens when the rod is placed next the negative pole, or in fact in any part of the field other than that very near the anode.

Various solid dielectrics have been used, all giving the same general result. Among them various kinds of glass, such as lead, soda and different varieties of enamel glass, some in the form of solid rods, others as tubes, even freshly drawn glass fibers of not over three-tenths of a millimeter in diameter, sticks of sulphur, rubber tubes, rods of ebonite, different varieties of wood and finally, as an extreme case, an ordinary silk thread stretched taut and approximately at right angles to the lines of force.

The increase in spark-length was usually very pronounced, often more than doubling the maximum length obtainable under ordinary conditions.

In most of my experiments the machine was kept constantly running at an approximately uniform speed, but this was simply for the purpose of holding the potential difference as nearly constant

as possible, and not at all essential to the general phenomenon. The sparks of extra length could easily be obtained after the machine had come to rest, provided they were taken before the potential had fallen too low.

The following are among the conditions tested.

1. Polished poles, jars belonging to machine and their outer coatings connected, inner coatings oppositely charged. Results excellent, best obtained.

2. Polished poles, jars belonging to machine but insulated from each other, inner coatings oppositely charged. Increase in spark-length decided, but volume of spark small.

3. Polished poles of machine oppositely charged, no jars. Increase in spark-length still evident, but volume of spark of course very small.

4. Polished poles, one connected to inner the other to outer coating of a single jar. Results good.

5. Polished poles, one or more jars of large capacity. Results pronounced, but spark-length never so long or so greatly increased as with the jars belonging to the machine.

6. State of atmosphere such as to give only very short sparks. Increase in spark-length decided, but not very great.

7. State of atmosphere such as to give unusually long sparks. Increase in spark-length not great, but still evident.

8. Poles separated very widely, beyond the length even of the forced spark. In this case a small spark left the positive pole when the glass or other dielectric rod was brought in proper position and disappeared along numerous little branches.

9. Negative pole polished, positive pole very rough, in fact the broken end of a metal rod. Under ordinary conditions this gave only pinkish brush discharges, except when the poles were very close together, when numerous small, white and somewhat noisy sparks passed. On placing a glass rod next the positive pole, the white and noisy sparks were more than doubled in size and length.

Various other more or less important combinations were made, but these are sufficient to indicate the general nature of the phenomenon, the interpretation of which is probably the same no matter what the conditions under which it is obtained.

Just what the correct and complete interpretation really is, I do not pretend to say, but the increase of the spark-length when the discharge is between two oppositely charged jars, insulated from each other seems to preclude the assumption that it can be due to oscillations of any kind.

Possibly though an explanation is found in the fact that the solid dielectrics used in these experiments have specific inductive capacities greater than that of air, and consequently produced corresponding local concentrations of the lines of force, which concentration extended to a greater or less extent to the surface of the electrode when the dielectric was in its immediate neighborhood. It may be that the increase in the spark-length was due to this local increase of the electromotive intensity, but in framing an hypothesis to explain it, the fact that this increase must be next the anode has to be kept in mind.

For the sake of binding these and other experiments more or less closely together, I suggest the following hypothesis:

The dielectric between two poles is most sensitive to electrical disturbances at points next the anode.

The following are some of the facts that seem to support this idea.

(*a*) The-spark length is increased by the presence of a glass rod say next the anode, but not affected when the rod is in any other part of the field.

(*b*) The greater the increase of the spark-length, the nearer the glass rod must be brought to the positive pole.

(*c*) The greatest increase of spark-length is obtained with a glass rod of a certain size, other things being equal. That is the rod must not be either to large or too small for best results.

(*d*) When glass fibers are used they must be brought very near the pole, usually nearly or quite in contact with it.

(*e*) The same phenomenon follows when insulated conductors are used instead of glass or other dielectric.

With the view of testing this idea I tried, among other things, the effects of dielectrics of widely different inductive capacities, but otherwise approximately equal. The substances used were:

(*a*) A small thin-walled test tube, empty.

(*b*) Similar tube part filled with water.

(*c*) Similar tube part filled with metal.

The results were not conclusive, the increase in the spark-length remaining nearly the same, with if anything, the advantage in favor of the tube part filled with water, but this was not sufficiently pronounced to warrant deductions. Again the fact that insulated metal rods act no better than glass rods does not seem to be fully in accordance with the above assumption.

These, however, are the only experimental objections I am aware of, and it may be that they are by no means fatal.

In further support of this hypothesis, we have the increase of spark-length caused by a small spark drawn from the negative pole of two oppositely charged leyden jars when their outer coatings are connected, and also Faraday's experiments on spark-lengths between spherical electrodes of different sizes.

In speaking of these experiments of Faraday, J. J. Thomson says: "We may express this result by saying that when the electric field is not uniform the gas does not break down so easily when the greatest electromotive intensity is at the kathode as it does when it is at the anode."

It will be noticed that the hypothesis used to explain these experiments is only an adaptation of this sentence of Thomson's. Whatever the real action of solid dielectrics in increasing spark-length may be, it is a phenomenon of the anode, and to that extent indicates a greater sensitiveness of the air about this pole to electrical disturbances.

It would be interesting, but probably of little use at present, to speculate on the cause of this localized sensitiveness in the dielectric. Possibly it is due to the formation of Grothus' chains and the clustering of different types of ions about the two poles.

ROUSS PHYSICAL LABORATORY, UNIVERSITY OF VA., April, 1900.

ELECTRICAL RESISTANCE OF THIN FILMS DEPOSITED BY KATHODE DISCHARGE. II.[1]

BY A. C. LONGDEN.

THE ELECTRICAL MEASUREMENTS.

THE apparatus used in making the tests to which these resistances have been subjected was such as to afford results of the highest attainable accuracy; and in all cases where accuracy was considered important no painstaking effort has been spared in securing it.

The Wheatstone bridge and rheostat used in the Ryerson Laboratory was one of Queen's, of the kind known as "Set No. 1," No. 80. The rheostat consists of five sets of coils aggregating 11,111 ohms, and guaranteed by the manufacturers accurate to $\frac{1}{50}$ per cent. The bridge is provided with five pairs of reversible bridge coils ranging from a pair of units to a pair of 10,000 ohm coils. These are guaranteed accurate to $\frac{1}{100}$ per cent. The temperature of the coils was determined to within one hundredth of a degree by means of an electrical thermometer placed inside of the box.

Resistances beyond the range of this bridge were measured by the direct deflection method. In applying this method a new form of water battery[2] devised and constructed for this work was found useful, convenient, and comparatively inexpensive. In the construction of this battery, instead of using a copper plate and a zinc plate, two copper plates are used, one of which is amalgamated with mercury containing a little zinc. This plate presents a zinc surface to the water and is therefore equivalent to a zinc plate in the cell, but it does not dissolve in the cell, nor become brittle and break off at the surface of the water as an all zinc plate does. The resistance of this cell may be varied by varying its size, or, within certain

[1] Continued from page 55.

[2] Electrical World, Vol. 31, p. 681.

limits, by the introduction into the water of a *small* quantity ($\frac{1}{10}$ of 1 per cent.) of sulphuric acid. If the cell is properly constructed and sealed it requires no attention whatever for many months at a time, and its electro-motive force is practically constant as long as it is not overworked. Of course it will polarize quite rapidly if used on anything but very high resistance circuits. It is not intended to furnish much current.

In the work done at Columbia University, for measuring resistances of more than a few thousand ohms it was found convenient to use a bridge of the slide-wire type in which a potentiometer of 100,000 ohms forms the two variable resistances. The third resistance was the one under test, while the fourth was a standard of 10,000, 100,000 or a 1,000,000 ohms. The rest of the set consisted of a sensitive Thomson galvanometer and a battery of from one to one hundred dry cells.

The potentiometer used was designed at Columbia University by the late Mr. Holbrook Cushman, and was constructed for the university by a mechanician employed for the purpose. The coils were standardized and adjusted at the University by Mr. H. C. Parker and his assistants. A fuller description of the instrument is given in Mr. Parker's admirable work on Electrical Measurements.

The 10,000 and 100,000 ohm standards were made by Elliott Bros. of London. The 1,000,000 ohm standard was one of the platinum film resistances under discussion, but its resistance was frequently measured and it was found to be perfectly trustworthy.

With this apparatus a resistance as high as 20 megohms may be measured to within one part in 10,000.

In making the measurements for determining temperature coefficients, the standards of comparison were kept in the electrical testing room in the Fayerweather Laboratory. The resistances under test were first measured in that room at the same temperature as the standards, and again after being removed to an underground constant temperature room, located just outside of the main wall of the building, and entered from the sub-basement. One arm of the bridge in the testing room was connected with a mercury commutator in the underground constant temperature room by a well insulated, heavy copper wire circuit of small but known resistance.

For all high resistance work this circuit is negligible. If the resistance to be measured is small enough to make it at all important, the resistance of the line is deducted. The temperature changes in the line itself are very slight, but of course the method is not used for measuring resistances so small that temperature changes in the line itself would produce appreciable errors in the results.

The temperature of the testing room is ordinarily about 20° C., and does not usually vary more than a degree during the course of a day. The temperature of the underground room is generally about 10° or 11° and changes very slowly. These temperatures were measured by means of well seasoned thermometers which have been carefully standardized by comparison with a thermometer accompanied by a Reichsanstalt certificate dated May 7, 1898.

This method of measuring resistances at different temperatures proved to be at once convenient and, what is much more important, reliable. Convenient, because no special precautions were necessary in order to maintain constant temperatures, and reliable because there could be no doubt about the temperature of a film or coil which had remained undisturbed in a constant temperature room for two or three days.

In measuring the temperature coefficients of film resistances, it was soon noted that all of the very thin films had negative temperature coefficients, while all of the thick ones had positive temperature coefficients, and that for a certain range of thicknesses the temperature coefficients were close approximations to zero. It was also noted that, unfortunately, some other factor besides thickness enters into the condition which produces zero temperature coefficients. This other factor may be, and probably is, density; for this is obviously the most variable property of the films. When it becomes possible to hold all the conditions under which the films are deposited in perfect control, it will, doubtless, be possible to produce zero temperature coefficients or temperature coefficients of any desired value by simply producing films of certain thicknesses. Even under present conditions the range of thicknesses for negligible temperature coefficients is not small.

Before proceeding further with this part of the subject it may be well to state that for convenience a standard length and breadth

were adopted for all films, so that, as the thickness was the only varying dimension, its relation to temperature coefficients might the more easily be discovered. Therefore, wherever in this article films are in any way compared, it may be assumed that they are films of equal length and breadth. The adopted total length of the glass plate is 7 cm., but as a centimeter at each end is covered with a *thick* film of negligible resistance for making the electrical connections, the length of the *thin* resistance film is 5 cm. The breadth is in all cases 15 mm.

A platinum film of the above dimensions thick enough to have a resistance of only about 400 ohms, has a positive temperature coefficient equal to 0.0003. The largest negative temperature coefficient measured was — 0.005. This was for a film so thin as to have a resistance of about 18 megohms. If the curve connecting temperature coefficients with thickness, were a straight line, zero temperature coefficients would occur at about one megohm, for films of these dimensions, as seen by the position of the dotted line in Figure 9. As a matter of fact, however, zero or negligible tem-

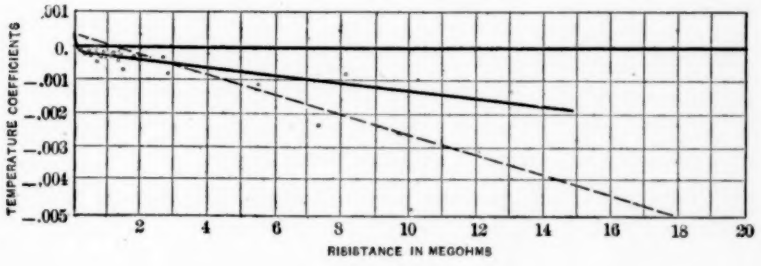


Fig. 9.

perature coefficients frequently occur in resistances of less than 5000 ohms, and they occur all along from that point to somewhere in the neighborhood of 100,000 ohms. It must also be remembered that the very large negative coefficient mentioned above is an extreme case. The large number of results indicated by the points between the axis of ordinates and the two-megohm line in Figure 9 indicates that for films of a certain density the real position of the curve is probably not far from that represented in the figure. Fortunately the range of low temperature coefficients, from a few thousand ohms up to about a megohm, is an exceedingly useful

one. It could hardly have happened to occur in a more desirable place.

Adjusting and Standardizing Film Resistances.

It is important to consider in connection with the question of temperature coefficients, a method of adjusting these resistances to a particular value. The method referred to is substantially the same as the one described by Mr. Fawcett. It was also used in this investigation as early as June 2, 1898. It consists of simply cutting the film into sections, as in Fig. 10, so as to increase the length and diminish the breadth of the conductor. In this way the resistance of a film may easily be increased ten fold or even much more than that, if necessary.

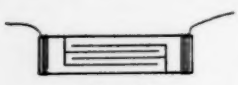


Fig. 10.

Of course a resistance may in this way be adjusted to any desired value, provided the final value is to be *higher* than the original value; but the importance of the method in connection with the question of temperature coefficients is in the fact that a film *thin* enough to have a resistance of several megohms is quite likely to have a rather large negative temperature coefficient (see Fig. 9), while a film of something less than 100,000 ohms resistance with a zero or negligible temperature coefficient may, by means of the method here described, have its resistance increased to the higher value without altering its temperature coefficient.

For making very high resistances with zero temperature coefficients it is advisable to use wider plates, so that the current may be made to traverse the length of the plate a greater number of times without making the conductor dangerously narrow.

Fig. 11.

The same result may be accomplished by bending a small glass tube or rod into the form shown in Fig. 11 and depositing upon it a film of such thickness as to have a zero or neglible temperature coefficient. In this case, however, the final adjustment to a required value is not quite so easy.

Aging and Protecting the Films.

The change in resistance which a film undergoes, quite rapidly at first, and less rapidly later, makes the "artificial aging," or "sea-

soning " process one of great importance. There are, however, no difficulties connected with it. It is the same process for films as for wire resistances. The films may be heated and cooled in the air, or they may be boiled in oil or in melted paraffin. In any case the process is rather a tedious one and should not be considered at an end until sometime after the point has been reached where no further change is noticeable. Otherwise the resistance will continue to undergo slight changes for a considerable length of time. The following table exhibits some of the results of perfect and imperfect artificial aging :

		Resistance in Ohms.			
Class A.	1	2265.8	2265.7	2265.8	2265.8
	2	37586.	37585.	37588.	37586.
Class B.	3	384.90	383.72	383.16	382.92
	4	9999.5	9992.5	9990.3	9989.4
	5	15722.	15697.	15685.	15681.
	6	23335.	23680.	23765.	23774.
	7	165200.	167680.	167880.	167930.
Class C.	8	84167.	83710.	83431.	83178.
	9	656030.	653600.	651620.	650110.
	10	4690000.	5008000.	5070000.	5080000.

The values given in the successive columns are for measurements about a month apart, and either made at the same temperature or reduced to the same temperature ; the temperature coefficients of all the films having been carefully determined.

The films in class (A) were thoroughly seasoned ; while those in class (B) were less perfectly seasoned and those in class (C) were very poorly seasoned. It will be observed that in class (C) the variations amount to as much as two or three tenths of 1 per cent. even during the third month. In class (B) the corresponding variations are only a few hundredths of 1 per cent. ; while in class (A) no change greater than could be attributed to errors of observation was detected during the entire duration of the test.

In subjecting a number of films to the process of artificial aging, a rather important relation between this process and temperature coefficients was observed. By keeping the films connected with

the Wheatstone bridge during the aging process, it was seen that the resistances did not continue to change in the *same direction* during the entire process. It was observed, first, that if the rapid change of resistance which occurs when the film is first placed in a hot bath is an *increase*, it will soon reach a maximum and then gradually *decrease* for a number of hours; second, that if the first rapid change is a *decrease*, the later and more gradual change will be an *increase;* and third, that if the resistance does not change at first it will not change at all. Now a film of the first kind always has a positive temperature coefficient, while one of the second kind always has a negative temperature coefficient. Of course the temperature coefficient of the third kind is zero.

The sign, and roughly the value of the temperature coefficient of a film resistance may be determined by holding a glowing incandescent lamp within about an inch of the film, while the latter forms one arm of a balanced Wheatstone bridge. If the resistance does not change at all the temperature coefficient is zero and the film does not need much artificial aging. Briefly stated, the changes in resistance which occur during the process of artificial aging, and the length of time required to bring the resistance to a permanent value, are proportional to the magnitude of the temperature coefficient. Therefore a low temperature coefficient is important, not only for its own sake, but also as a means to an end. Films having low temperature coefficients are very easily seasoned and extremely reliable after seasoning.

It is probably true that in films there are not as many causes at work to create a necessity for artificial aging as in the case of wires.

It has recently been found by C. de Szily[1] that if a wire be subjected to *torsion* its electrical resistance increases, and that if the torsion is maintained the resistance very slowly diminishes with time. Also that a wire, which, after dextral torsion, has been relieved and then twisted by sinistral torsion to the zero position, diminishes in resistance more quickly that when the twist is maintained. Also that when a wire has been twisted beyond its elastic limit, and has been allowed to untwist itself, subsequent changes of

[1] Comptes Rendus, Vol. 128, pp. 927–930.

resistance for torsion are less than those which occur during the first twist. It is probable that stretching, bending and even winding upon a spool all produce a similar condition in wires.

After the artificial aging process is at an end it is important that the films be protected from the air in order that no new conditions may arise to produce changes in resistance. They may be enclosed in glass tubes from which the air is exhausted, they may be simply imbedded in paraffin, or they may be coated with varnish prepared by dissolving India-rubber in carbon bisulphide.

Thickness of Films.

In consideration of the relation which evidently exists between thickness of films and temperature coefficients, it seems desirable to be able to measure the thickness of the films.

It is to be regretted that no method of making an exact determination of the thickness of the *very thin* films has been discovered. Miss Stone calculated the thickness of her films from their weight and area, assuming that the density was the same as that of silver in its ordinary condition. However correct or incorrect this method may have been for silver films deposited from aqueous solutions, it certainly would not be very useful in determining the thickness of films of such varying compactness as those deposited by kathode discharge. Furthermore, many of the films considered in this research are entirely too thin to produce any impression whatever upon the most delicate balance; so that both density and weight are unknown quantities. Even if an exact determination of weight could be made, it would be of doubtful value, because of the varying and always indefinite amount of air and other gases absorbed by the film and weighed with it. The property of absorbing and occluding gases does not belong to platinum alone. It is possessed in some degree by a large number of metals.

For this and other reasons it was deemed desirable to attempt to determine the weight of a few films by the methods used in quantitative chemical analysis. Silver was the metal chosen for these experiments, because of the very delicate and exact existing method for its quantitative determination. After the films were deposited, they were converted into silver nitrate and then subjected to the

volumetric process known as Gay-Lussac's method. The quantity of silver in these films was so small that although the method of analysis used was the one which Fresenius describes as " The most exact of all known volumetric processes," yet it was not possible in any case to make an exact determination of the amount of silver in a *transparent* film, and if the films were really *thin* it was not even possible to *detect* any silver in the solution. These facts are mentioned as forcible illustrations of the exceeding thinness of the films.

The assumption upon which Mr. Fawcett bases his method of calculating thickness is certainly unwarranted; for in very thin films, the ratio of the measured resistance to the calculated resistance is unquestionably very much higher than it is in thicker films.

Professor Michelson's interferometer furnishes a direct method of measuring the thickness of comparatively *thick* films, but that is not the important part of the problem; for even a thick film, in the sense in which the terms thick and thin are here used, is only one or two tenths of a wave length thick. However, a few comparative experiments were made upon thick films, determining the thickness by means of the interferometer and also from weight and area. In some cases the results were fairly concordant, indicating that in these cases the density was about normal.

A silver film which was just transparent enough to reveal the outline of a luminous gas jet was used in making one of these comparisons. A portion of the film was removed from the glass and the optical difference of path between the surface of the glass and the front surface of the film, was measured by means of the interferometer. The displacement of the interference fringes due to this difference of path was about .3 of a fringe. (Sodium light.) The thickness of the film is therefore about 0.00009 mm. The thickness of the same film, calculated from its weight, 0.0008 g., and area, 7.38 square centimeters, assuming a density of 10.55, was 0.00010 mm. A platinum film somewhat more transparent was found by the interferometer method to have a thickness of 0.00006 mm., while its thickness, calculated from its weight and area was 0.000063 mm. This, however, does not prove that the density of thin films is the same as that of thick ones or even that the density is the same for all films of the same thickness.

It must be remembered that both of the films just considered were *thick* films. The utter uselessness of attempting to weigh very *thin* films is illustrated by the fact that a platinum film 15 mm. wide and 5 cm. long, weighing only 0.00025 g. has a resistance of only about 2000 ohms. What would be the weight of a film thin enough to have a resistance of several megohms?

Photometric methods are of some service, for they at least give an indication of the amount of metal in a film which is too thin to be weighed, but the results obtained by this method do not agree to within 10 per cent.

There is yet one method of estimating thickness to be considered.

It seems reasonable to suppose that the rate of deposition must be uniform under uniform conditions, and that therefore the thicknesses of films ought to be directly proportional to the times required for their deposition; provided only that none of the conditions are allowed to change during the process. The electro-motive force, current and distance are under perfect control. The frequency of the Wehnelt interrupter is not very difficult to control if the temperature is kept constant by means of a cold water circulation. Even the vacuum may be kept within reasonable limits if very great care is exercised during the first part of the process, while the occluded gases are being expelled. It is therefore believed that the time element in the deposition of a film may be made the most reliable basis for estimating thickness; and it is this element that has been trusted more than any other in the estimates of thickness which have been made during the progress of this research.

Choice of Materials.

In the choice of a metal for the production of film resistances, permanence has been the chief consideration, and platinum, all things considered, is believed to be the most suitable metal. Gold is doubtless just as permanent as platinum as far as its ability to resist chemical action is concerned; but its molecular condition does not settle down to a permanent value as that of platinum does. H. L. Callendar in his work "On a Practical Thermometric Standard"[1] condemns gold as a material on the ground that there is a slow but

[1] Phil. Mag., S. 5, Vol. 48, pp. 519–547.

constant change of zero in a gold thermometer. This of course, means a slow but constant change of electrical resistance at a certain temperature. It is for this reason that gold is not a suitable metal for resistance standards.

A number of other metals have been experimented upon, but none of them seemed to possess any advantage over platinum.

SUMMARY OF RESULTS.

The conclusions drawn from this investigation, briefly stated, are as follows :

(1) The process of depositing metals by kathode discharge as illustrated by figures 3, 4 and 5, is probably simple *distillation*, in which the vaporization of the kathode is largely due to its electrification.

(2) The temperature coefficients of the very thin films are negative, and for films within a certain range of thicknesses, the temperature coefficients are approximately zero.

(3) The necessity of artificial aging is proportional to the magnitude of the temperature coefficient.

It may be said in general that the method herein described is capable of producing standard electrical resistances of a very high degree of precision and of any desired value, from a few ohms up to several megohms ; that these resistances may be made of pure metals—simple elementary substances—and that they are therefore free from the possibility of decomposition changes ; that the metals may be those least likely to enter into chemical combination with other elements ; and that in addition to these valued qualities as pure metals, they possess the only desirable qualities of alloys, namely high specific resistance and low temperature coefficients.

In closing this dissertation I wish to express my sincere thanks to Professors Rood and Hallock, of Columbia University, for their kindly interest in my work and for the many helpful suggestions which they have given me. Also to Professors Michelson and Stratton, of the University of Chicago, for valuable assistance during the early part of the investigation.

FAYERWEATHER PHYSICAL LABORATORY,
COLUMBIA UNIVERSITY, March 31, 1900.

EFFECT OF MAGNETIZATION UPON THE MODULUS OF ELASTICITY.

By James S. Stevens.

III. Elasticity of Traction.

IN previous papers the relation between magnetization and the moduli of flexional and torsional elasticity has been discussed. The present experiment deals with elasticity of traction. My assistant, Mr. Stanley Sidensparker, undertook the work and carried it on with occasional assistance.

The form of the apparatus may be understood from the figure.

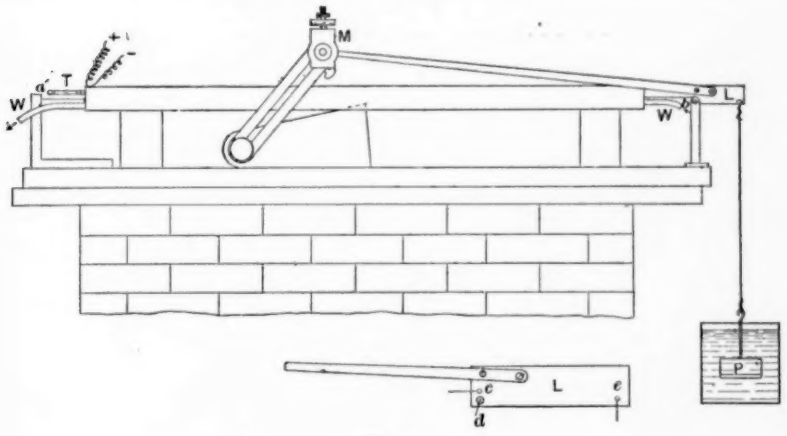


Fig. 1.

A coil of wire was wound on a brass tube through which passed a stream of running water, in which was carried the wire to be tested. One end of the wire was fastened to a rigid iron support and the other was attached to a bent lever with a multiplying power of about 11. At the other end of the lever were attached weights which gave a tension on the wire of 5.39×10^6, 10.78×10^6 and 19.6×10^6 dynes respectively. The changes in length were communicated to another arm of the same lever, which multiplied the

effect 110 times. The free end of this lever moved under the field of a micrometer microscope. A thermometer whose bulb was inserted in the stream of water completed the arrangement.

DATA.

Length of coil	131.1	cm.
No. of turns of wire	1612	
Diameter of wire	0.13	cm.
Length of wire tested	175	cm.
Diameter of wire A	0.73	mm.
Diameter of wire B	0.98	mm.
Tension weight A	5.5	kgm.
Tension weight B	11.0	kgm.
Tension weight C	20.0	kgm.
Multiplying power of first lever	11	
Multiplying power of second lever	110	
Microscope constant 28.33 turns per min.		
Current	2.2–7.6	Amp.

The procedure consisted in setting the cross wires of the microscope on some point for zero, then turning on the current and noting the deflection of the end of the lever. The tables which follow give the current, zero readings, the difference, and the change in the length of the wire reduced to centimeters.

TABLE I.

Wire B. Tension B.

Current.	Initial reading.	Final reading.	Difference.	Change in length in cms.
	42	88	46	15×10^{-6}
	75	18	43	14 "
	79	10	31	10 "
	68	16	48	15 "
	82	26	44	14 "
	79	15	36	11 "
2.12–2.18 amps.	59	92	33	11 "
	41	80	39	12 "
	4	36	32	10 "
	81	14	33	11 "
	56	5	49	15 "
	51	87	36	11 "
	41	74	33	11 "

The above table shows complete readings in order to set forth the degree of constancy which the experiment afforded. This was about an average set of readings. The tables which follow this give the mean only of a number of readings, usually about 12.

TABLE II.

Wire B. Tension B.

Current amps.	Change in length cms.	Current amps.	Change in length cms.
2.62	13×10^{-6}	5.30	26×10^{-6}
3.95	16 "	6.50	48 "
4.50	20 "		

TABLE III.

Wire B. Tension A.

Current amps.	Change in length cms.	Current amps.	Change in length cms.
2.6	6.0×10^{-6}	4.2	12.8×10^{-6}
3.5	9.0 "	4.7	16.0 "

TABLE IV.

Wire A. Tension A.

Current amps.	Change in length cms.
4.0	12.8×10^{-6}
7.7	25.4 "

TABLE V.

Wire B. Tension C.

Current amps.	Change in length cms.	Current amps.	Change in length cms.
6.9	15.0×10^{-6}	7.6	19.2×10^{-6}
7.1	16.1 "		

TABLE VI.

Wire B. Tension B.

Current amps.	Change in length cms.
6.8	10.2×10^{-6}
8.0	11.2 "

From the tables we may draw the following conclusions:

1. The modules of elasticity of traction *increases* when the body is magnetized.

2. The increase is fairly proportional to the magnetizing force.

3. With the same wire the change in the modulus varied with the stretching weight.

4. With wires of different cross sections, stretched by the same load, the change in modulus was greater with the smaller wire.

It was considered that errors due to change in temperature were entirely avoided or reduced to negligible quantities. The stream of water in which the wires were placed kept the temperature so constant that changes greater than 0.1 degree did not occur in fifteen minutes. Moreover the change in the length of the wire due to magnetization was noted in about fifteen seconds. It may therefore be assumed that temperature changes during the time in which the observations were made were about of the order of $\frac{1}{600}$ of a degree F. This was about $\frac{1}{3}$ the value of the smallest observed change. The principal test for absence of temperature effects, however, was the fact that as in former experiments already noted, the changes in length due to magnetization are sudden and easily distinguished from the temperature effects which are creeping. Whatever changes may be due to temperature must of course be added to the results above given since they take place in the opposite direction. The same remarks will apply to changes in the length of the wires due to magnetization alone.

IV. ELASTICITY OF COMPRESSION.

In this experiment the apparatus was constructed, and the measurements taken by Mr. H. G. Dorsey of the department of physics at this University.

The figure shows the arrangement of apparatus. The bar was fastened firmly at one end and moved a lever arm when changing in length. The other arm of the lever moved under the field of a micrometer microscope. The compressing force was measured by a spring balance. The whole apparatus was bolted rigidly to the bed of a planer, so that there was little chance for slipping or friction. The knife edges and bearings were of hardened steel and variation of pressure was applied by moving the bed plate of the planer.

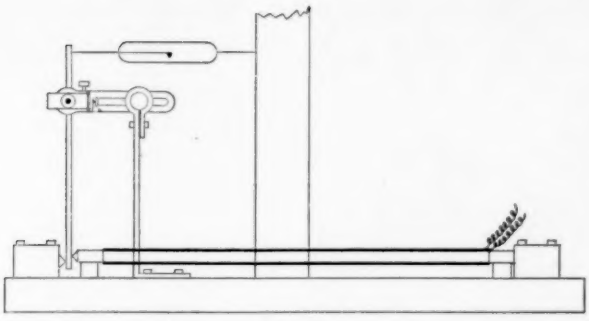


Fig. 2.

DATA.

Length of rod.....134.5 cm.
Diameter of rod ..... 2.54 cm.
Resistance of coil ..... 3.3 ohms.
Wound with No. 16 double cotton covered magnet wire 1830 turns.
Ratio of power arm.....1 to 120.
Ratio of index .....1 to 106.7
Constant of micrometer microscope, 28.33 turns to 1 mm.

The tables which follow are self explanatory.

	270 kilos.		540 kilos.		1080 kilos.
C.	*change.*	*C.*	*change.*	*C.*	*change.*
.5	.00038+	.5	.00014+	..	.00030+
1.	.00073+	1.	.00031+	2.	.00034+
1.5	.00068+	2.	.00083+	4.	.00023+
2.	.00051+	3.	.00028+	6.	.00000
2.5	.00021+	3.5	.00025+	8.	.00021—
3.	.00019+	4.	.00012+	10.	.00029—
3.5	.00009+	5.	.00011+	15.	.00052—
4.	.00000	6.	.00000	23.	.00180—
4.5	.00002—	8.	.00018—		
5.	.00017—	10.	.00034—		1350 kilos.
6.	.00018—	17.	.00130—	*C.*	*change.*
7.	.00032—	23.	.00139—	1.	.00033+
8.	.00039—			2.	.00019+
17.	.00112—			4.	.00015+
20.	.00145—			6.	.00000
23.	.00154—			10.	.00012—
				15.	.00043—
				22.5	.00112—

Current in Amperes. Changes in mm. + Increase in length of bar.
— Decrease in length of bar.

When these results came to be plotted it was found that very little difference could be detected for changes in compressing force varying from 270 to 1350 kilos. It is noticeable also that the curves

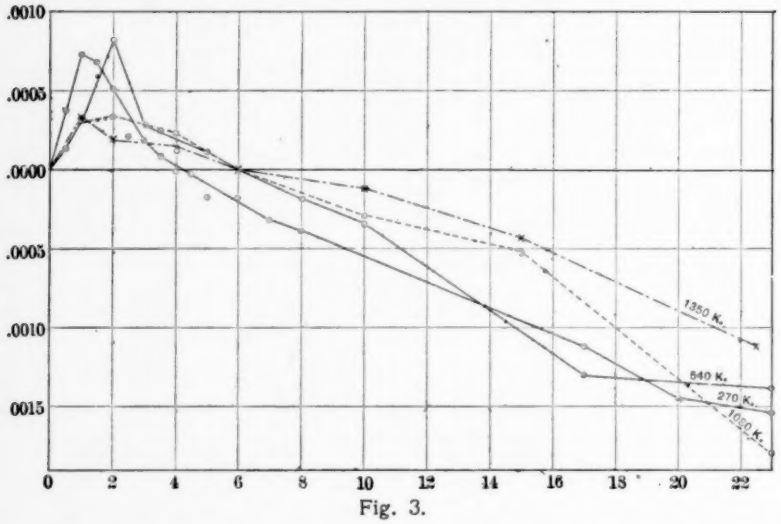


Fig. 3.

resembled those of other experimenters who have tested the changes in length of a bar when magnetized. They are particularly similar to those obtained by Dr. L. T. More. (See PHYSICAL REVIEW, November–December, 1895.)

It may be said therefore that within the limits of this experiment magnetization does not change the elastic modulus for compression. If it had been possible to apply a pressure nearer to that of the elastic limit of the metal, the results might have been different.

Of the four experiments described in this JOURNAL, three have shown clearly that magnetization increases the elastic modulus in case of torsion, traction and flexion. We began these investigations with no theory as to the results. An experiment along a different line previously performed, seemed to indicate that magnetization did not change the modulus; another (*Phil. Mag.*, June, 1899), suggested that the modulus by flexion was decreased.

The behavior of magnetized bodies offers an attractive field for investigation and cannot fail to throw light on the general theory of magnetization, and indirectly on the constitution of bodies.

THE UNIVERSITY OF MAINE.

SPECTRA OF MIXTURES.

By Carl J. Rollefson.

AMONG the results obtained from a photographic study of arc spectra, the following may be of some interest. The aim of the experimenter was to note the change, if any, in the spectrum of a metal when a salt of this metal was introduced into the arc together with the salt of some other metal.

The apparatus used were about the same as those used by Foley,[1] and described by him in the Physical Review. A Rowland concave grating of 6½ feet radius and 14,438 lines to the inch, was placed upon a Brashear mounting. The light from the arc lamp used was reflected upon the slit by a concave mirror of ten-inch aperture and twelve feet radius. In Fig. 1, *L* represents the arc

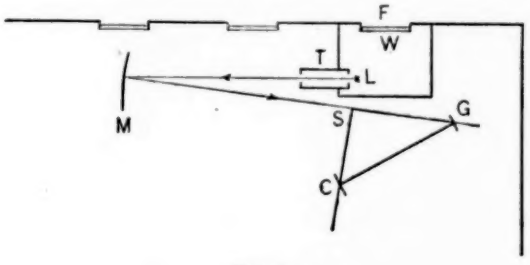


Fig. 1.

lamp, *M* the mirror, *S* the slit, *G* the grating, *C* the camera box. The slit, *S*, had the usual adjustments for width, length and direction. The width used varied from .015 mm. to .02 mm. The room was not darkened, but the space from *S* to *G* and from *G* to *C* was inclosed so as to exclude all light except what entered through the slit *S*. The tube from *S* to *G* consisted of a number of metal rings placed crosswise at intervals along the track, and about these rings black cotton flannel was wrapped. In order to

[1] Arc Spectra, Arthur L. Foley, Physical Review, Vol. 5, p. 129, 1897.

prevent the ultra-violet rays of the first spectrum, and also the rays from spectra of higher orders from falling upon the sides of the tube leading from *G* to *C*, from which they would be reflected upon the plate, a tapering box, shown in Fig. 2, was used. Along the

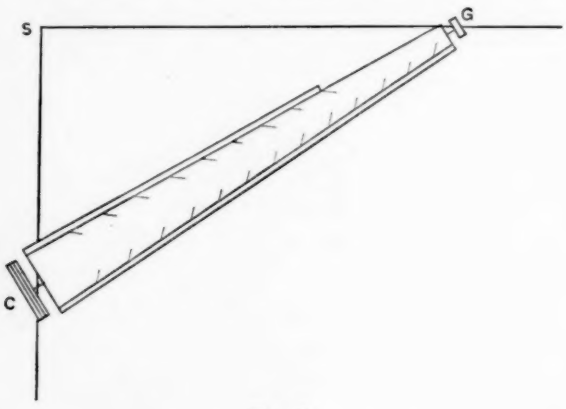


Fig. 2.

inside walls of the box were fastened strips of tin, and the whole interior was painted black. The camera box was constructed for 2 x 8 inch plates, curved to 6½ feet radius. In order to conform to the grating the plates should have been bent to 3¼ feet radius, but the plates would break when bent to this curvature. That the spectrum remains a normal one when the plates are bent to a radius of 6½ feet, is shown by Foley in the article already referred to.

The metallic salts experimented with were at first introduced into both carbons, but later only one carbon was filled, and this was usually made the positive electrode. A pair of carbons was used for each salt or mixture.

All of the exposures were made in the primary spectrum in the region where the actinic effect is greatest, from $\lambda = 3200$ to $\lambda = 5000$.

I.

On a plate containing the spectrum of strontium and lithium were noticed some lines that did not seem to coincide with any known lines. As these same lines were obtained when a mixture of the chlorides of strontium and potassium was used instead of strontium oxide and lithium carbonate, and did not appear under any circum-

stances when strontium was not introduced, the conclusion was drawn that these lines were due to strontium. The wave-lengths were found to be 3780.68 and 3807.51. Afterwards it was found that Rydberg, by the aid of his formula for the strontium series, had predicted a group of three lines in the cyanogen band at $\lambda = 3800$, and that Kayser and Runge[1] had then studied the spectrum of strontium again, introducing carbon dioxide into the arc to weaken the cyanogen band, and had discovered the triplet at λ 3780.58, 3807.51 and 3865.59.

It will be noticed that the two lines on my plate correspond to two of the lines found by Kayser and Runge, and the third line $\lambda = 3865.59$ was found immediately after reading the account of their experiments.

Besides these three lines there are six more that have not been recorded before, that seem to be due to strontium. The wave-lengths are 4139.89, 4313.64, 4701.29, 4704.42, 4707.65, 4714.42. The reasons for believing these to be due to strontium are that they have not been found except where strontium has been introduced into the arc, and they have the same general appearance as the known lines of strontium.

Besides these lines there is also a band extending from $\lambda = 4400$ to $\lambda = 4410$, which seems to correspond to the magnesium oxide bands, but is very weak in comparison with them. It is probably due to strontium, or strontium oxide, as it has been found only on plates containing the spectrum of mixtures of which strontium oxide, or chloride, formed a part.

II.

In her study of arc spectra Miss C. W. Baldwin[2] found that there is a decided difference in the spectrum of the several sheaths of the arc, and that "the difference is largely due to a fading out of the carbon bands and of all lines from the positive carbon, and an increase of intensity in the outer sheaths of those lines which are most brilliant near the negative carbon." This was later inves-

[1] Beiträge zur Kenntniss der Linienspectren. H. Kayser u. C. Runge, Wied. Ann. 52, 1894.

[2] PHYSICAL REVIEW, Vol. III., Nos. 17 and 18, 1896.

tigated by Foley,[1] whose results do not agree with those of Miss Baldwin. He found that many of the metallic lines are relatively much stronger in the outer region of the arc, than other lines due to the same element, but did not find any that have a maximum of intensity in the outer sheaths.

Some of the plates obtained in the present investigation are of some interest in this connection. Among the metals experimented with were bismuth and lead. In studying the spectra of these two metals the horizontal arc was used. The center of the arc was focused on the middle of the slit so that the ends of the slit extended out through the outer sheaths of the image of the arc. The object of the experiment being to see if the lines of either bismuth or lead were influenced in any way when a mixture of the salts of these two metals was introduced into the arc, three sets of carbons were prepared. Two of them contained the salts separately, and the third contained the mixture. The salts used were nitrate of bismuth and peroxide of lead. When the plates had been exposed and developed it was found that strontium had been present as an impurity either in the carbons or in the salts used, and that the strontium line at $\lambda = 4607.52$ was not continuous. It was not visible in the middle, while both ends were strong and extended beyond the carbon lines. This result was afterwards verified.

The effect of introducing strontium with bismuth and lead was then tried. When only a small quantity of strontium is introduced the line is stronger at the ends than in the middle, but the plates seem to indicate that as the quantity of strontium introduced is increased, the middle of the line increases in intensity relatively more than the ends.

The barium line $\lambda = 4554.31$ is also stronger in the outer sheath under the same conditions. It does not, however, on the plates obtained, extend out in both directions as the strontium line does, but only at the end produced by the rays coming from the flame above the arc.

It is doubtful if any of the calcium lines mentioned by Miss Baldwin are affected by bismuth or lead.

[1] PHYSICAL REVIEW, Vol. V., p. 129, 1897.

DESCRIPTION OF A NEW ATWOOD'S MACHINE AND STOP CLOCK.

By Will. C. Baker.

THE Atwood's machine and stop-clock recently built in the mechanical laboratories of this university for use in the physics department, presents some novel features that are of interest.

The clock consists of a uniformly rotating shaft carrying a loose sleeve to which are rigidly attached a wheel of one hundred teeth and a pointer or hand that indicates the angular position of the

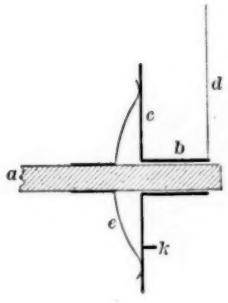


Fig. 1.

Section of Shaft and Sleeve showing frictional connection.

a, uniformly rotating shaft.
b, loose sleeve.
c, wheel of 100 teeth.
d, pointer or hand.
e, light spring forming friction clutch.

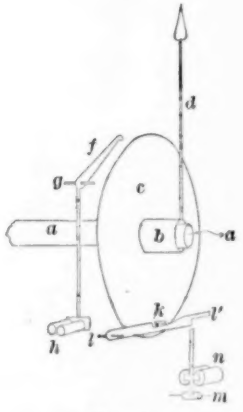


Fig. 2.

Perspective view of release lever, stop pawl, and magnets.

f, stop pawl.
i, release lever.
k, peg.
h and *n*, electromagnets.
g, *l* and *m*, pivots.

sleeve on a fixed dial graduated with one hundred divisions. (See Figures 1, 2 and 3.)

Rigidly attached to the shaft and pressing lightly against the back of the toothed wheel is a spring that communicates the motion

of the shaft to the sleeve and hand except when these are held by catches, in which case the spring slips over the surface of the wheel. Immediately over the toothed wheel is a pawl, *f*, (Fig. 2) pivotted at *g*, which, by means of an electromagnet, *h*, may be caused to fall into one of the notches between the teeth of *c*, and so arrest the motion of the hand. The wheel *c* carries also a small peg situated at *k*, diametrically opposite to the pointer *d*. A second catch pivotted at *l*, and notched as shown, is supported by a vertical piece hinged at *m*, and arranged so that when it is in the position shown, *k* catches in the upper notch of the release lever, stopping the hand at the zero of the dial. Now, when the electromagnet, *n*, is energized, the support is drawn back, the release lever falls free of *k* and the hand and sleeve, being very light, take on the motion of the shaft practically instantaneously.

In the Atwood's machine itself the cylinder that is to descend with its rider is supported by a small horizontal platform, *p*, as shown in Fig. 3, and in detail in Fig. 4. The energizing of the

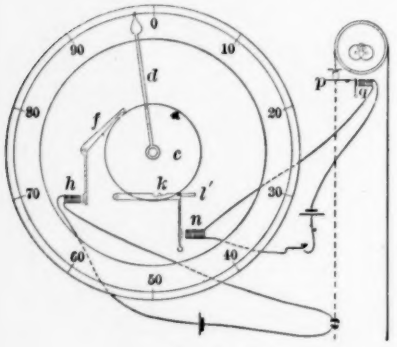


Fig. 3.

Diagrammatic view of connections. *p*, dropping platform.

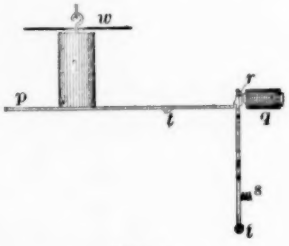


Fig. 4.

Detail of dropping platform.
q, electromagnet.
r, armature.
s, spring.
t, *t*, pivots.
w, cylinder and metal rider.

electromagnet *q*, as may be easily seen, permits the platform to drop and the cylinder to fall. This electromagnet is put in series with a key, a battery cell, and the electromagnet *n* of the clock. The closing of the key releases the clock hand and the following cylinder at the same instant.

The ring at the base of the machine consists of two half circles

of lead held by an insulating support. These are connected to the opposite poles of a battery cell, by wires, one of which has in current the electromagnet *n* of the clock. When the metallic rider falls across the ring it completes the circuit, throwing the pawl, *f*, into a notch of the wheel, *c*, and the hand is stopped at once. Fig. 3 shows these connections diagrammatically.

The regulation of the clock-shaft *a* (driven by a weight through a train of wheels) is affected as follows: A toothed wheel rigidly attached to the back end of *a* works into an endless screw on a vertical shaft, that bears a heavy cross bar supporting fans that may be set at any angle to their path. (See photograph, figure 5.) As this regulator has a high moment of inertia the change of the angular velocity of the main shaft, *a* is very slow [1] and the stopping of the motion of the sleeve, even for a few seconds produces but a very slight effect. The following method of reading is therefore adopted: The shaft is allowed to attain a state of uniform rotary motion, the sleeve moving with the shaft. The cylinder and rider of the Atwood's machine are raised into position. Just after the stop *k* has passed the notch, the release lever is raised [2] and the vertical piece springs forward and supports it. As soon as the hand, at the beginning of the next revolution, is checked by *k* striking the notch, the observer closes the key and immediately the hand starts on again from zero and the cylinder with its rider begins its fall. When in its descent the rider falls across the ring, the second circuit is closed and the hand stops. The reading is now taken and the rider removed at once. Sometimes, especially for rapid falls, the rider bounces from the ring, merely checking the hand for an instant, but even in this case no difficulty has been found in reading.

The cylinder is raised to place again and all is ready for a second observation.

The machine has not been used to determine the velocity produced by a certain fall [3] but to show that the acceleration produced in a given mass varies directly with the force acting, and that the

[1] This may be measured by a hand fixed to *a*.

[2] This is done (from the outside of she case) by a small lever (seen in figure 5).

[3] This would require a mere change of connections and a second adjustable platform.

acceleration produced by a given force varies inversely with the mass acted on.[1]

Forty-one consecutive readings of a certain fall were made to test the constancy of the apparatus. The time value of one division (S)

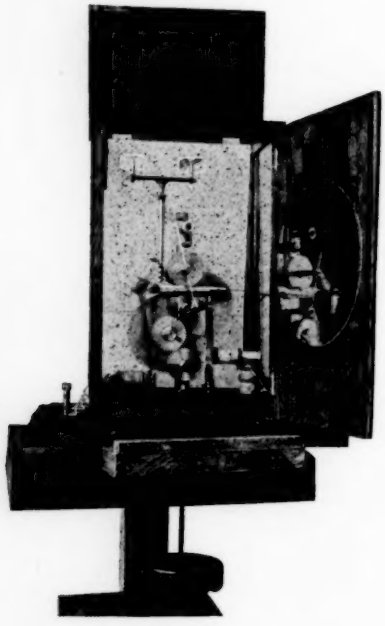


Fig. 5.

was 0.016 seconds, and the values obtained were: 327 S, 14 times; 328 S, 17 times; 329 S, 6 times; 330 S, twice; and 331 S, twice. A set of thirteen readings (all 197 S or 198 S) was made, as in the

[1] By using two riders A and B and four cylinders M, N, x and $\bar{y}$ having the mass relations $A > B$, $M = N$, $x = y$ and $M > x$. Now if $A + M$ falls, raising $B + N$ the force acting is $(A - B)g$. If $A + B + M$ raise N the force is $(A + B)g$ and the mass moved is the same as before. The force ratio $\frac{A - B}{A + B}$ should be equal to the acceleration ratio $\frac{a_1}{a_2}$ which is given by $\frac{t_2^2}{t_1^2}$, t_1 and t_2 being the lines of falling the same distance in the first and second cases.

Again if $A + x$ be made to raise $B + y$ the forces in the first and third cases are the same but the mass ratio is $\frac{M_1}{M_3} = \frac{A + B + M + N + W}{A + B + x + y + W}$, W being the correction for the wheel of the Attwood's machine. The mass ratio should be the inverse of the acceleration ratio. This method is given by Ames and Bliss.

second case of the preceding footnote, with the result $\frac{F_1}{F_2} = 0.364$ and $\frac{a_1}{a_2} = 0.362$, a discrepancy of about the half of one per cent.

The apparatus was designed by Professor N. F. Dupuis, Dean of the Practical Science Faculty, and was constructed under his supervision by students of that department.

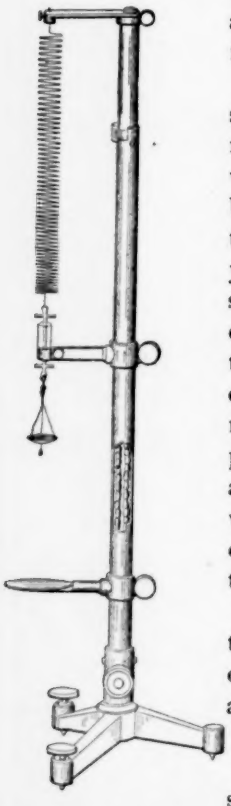
The photograph, Figure 5, shows the clock and driving weight, the dial and shaft hand being removed. The peg K and release lever in its raised position may be seen rather better as reflected in the clock face than directly. The screw regulating the stop pawls, may also be seen. To the spectator's sight is visible part of the small lever for raising the release lever from the outside of the case.

PHYSICAL LABORATORY, QUEEN'S UNIVERSITY,
KINGSTON, ONT., May 7, 1900.

A NEW FORM OF THE SPIRAL SPRING BALANCE.

C. E. LINEBARGER.

THE determination of weight by means of a measurement of the elongation of a spiral spring has become very common and "spring scales" of all sorts and sizes abound. The reason for this lies in the circumstance that such instruments are self-contained, have no easily separable parts and are very simple to read, a change of length alone being the measure of the force.



In scientific laboratories the form of the spiral spring balance devised by Jolly is in general use, mostly for the finding of specific gravities and the verification of Hooke's Law. Although the Jolly balance is so popular and has been in use for over thirty years in essentially the same original form, yet it must be conceded that it has some rather undesirable features. Thus, when it is moved, the spring often flies about in a most disagreeable manner; there is no arrangement for steadying and dampening the spring's vibrations; the platform can be moved only up and down; the reading of the position of the head on the mirror-scale is trying and hard on the eyes; it is almost impossible to use with it any accessory apparatus because the latter cannot easily be adjusted to follow the movement of the bead.

The present modifications in the construction of the spiral spring balance avoid to a considerable extent the undesirable features of the "Jolly" and give a wider range of usefulness to this style of balance.

Upon an iron tripod base provided with levelling screws is fixed a nickel-plated brass tube, within which slides a closely-fitting tube bearing a millimeter scale (500 mm.). This telescoping tube may be moved up and down by means of an endless chain running around two cogged shafts fastened near the ends of the outer tube, the lower one being attached to a milled head. The

upper end of the outer tube is cut out a little and bears a vernier. A third tube (10 cm. long), sliding in the telescoping tube and provided with a clamp, is for the purpose of lengthening the apparatus in case a spiral spring of extra length is used. It bears a horixontal arm for the suspension of the spring.

At the lower end of the spring is hung a piece of aluminium foil, cut in the shape shown in the figure. It plays in a glass tube held by an adjustable clamp. By this arrangement only a slight up and down or sideways movement of the spring is possible. One-half of the foil is blackened and a fine line is etched around the glass tube. The instrument is adjusted for reading by bringing the foil so that the line of division between its blackened and uncolored parts may coincide with the line around the glass tube. This reading device has proven to be very convenient and accurate. The platform is capable of sideways or vertical motion and moves very smoothly.

This instrument has been found applicable to a variety of experiments. Thus, besides being adapted to the verification of Hooke's Law and to the finding of specific gravities, it may be employed together with some simple accessory apparatus to illustrate the cohesion of liquids (a glass or metallic disk being used), to measure surface tension (a film of liquid being drawn up by means of a wire or foil frame) and to ascertain the relative intensity of magnetization in various parts of a bar-magnet by the "test nail method." With suitably wound coils and an iron core, ammeters and voltmeters of the Kohlrausch type may be readily set up, and by suspending from the spring a piece of copper foil immersed in a solution of copper sulphate, a voltameter may be arranged with which current strengths may be ascertained without removing the electrodes from the electrolyte or breaking the circuit. Also, with a steam calorimeter (serviceable ones may be made from lamp chimneys, glass tubings and corks) latent heats of vaporization and fusion and specific heats may be found.

APPARATUS FOR DEMONSTRATING BOYLE'S LAW.

BY F. J. ROGERS.

WHILE making use of an inclined tube of mercury for measuring pressure, it occurred to me that an apparatus based on this principle might be used with advantage for verifying Boyle's Law.

Fig. 1.

$Ac = 50$ cm.
$bc = 70$ cm.
$bB = 30$ cm.
$Aa = 13$ cm.

A glass tube 3 mm. internal diameter and 160 cm. long was bent into the form illustrated by Fig. 1. The tube was partially filled with mercury, and while lying in a horizontal plane the end A was sealed, the end B being left open. The apparatus in its simplest form was now ready for use.

If the bore of the tube is uniform the volume of confined air is proportional to the length Aa. And while the tube is lying in a horizontal plane the pressure to which the confined air is subjected is equal to that of the atmosphere or to H cm. of mercury. If the tube be now placed in a vertical plane as in Fig. 2, the volume of confined air is reduced

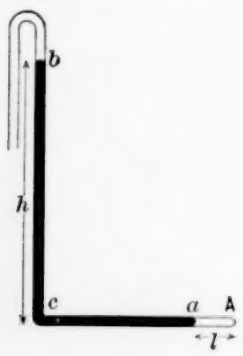


Fig. 2.

Fig. 3.

while the pressure is increased by the column of mercury bc, so that it becomes $H + h$ cm. of mercury. If the tube be now turned 180° about

a horizontal axis it will be in the position illustrated by Fig. 3. In this case the volume of confined air has increased, while the pressure is less than the atmosphere by an amount represented by the column of mercury of height h (see Fig. 3). In any intermediate position, the part of the tube bc being inclined to the vertical, the pressure to which the confined air is subjected is $H \pm h$ in which h is the *vertical* distance of the free surface of the mercury column above or below the horizontal tube Ac.

The following table gives the first set of data taken with the first apparatus constructed. The dimensions of this apparatus were approximately as given in Fig. 1.

Volume of Air $= Aa = V$	Vertical height of $bc = h$	Pressure $= H + h = p$.	Pressure $\times$ volume $= p \times v$.
42.0 cm.	−41.7 cm.	32.0 cm.	1344
31.5	−31.0	42.7	1345
27.1	−24.3	49.4	1339
21.6	−12.0	61.7	1333
18.1	00.0	73.7	1334
13.7	+24.4	98.1	1344
10.3	+57.7	131.4	1353

Atmospheric pressure from barometer, $H = 73.7$ cm. of mercury.

In the last column of the table the numbers represent the product of pressure and volume. The average deviation from the mean value is less than ½ per cent. which is very satisfactory in the introductory laboratory work for which this experiment is suitable.

The advantages of this apparatus are: (1) the same tube and mercury may be used many times in succession without getting dirty; (2) the apparatus consists of a single piece of glass without rubber tubing and joints to leak air; (3) a large range of pressures, both greater and less than an atmosphere, is obtained combined with a very small cost for apparatus and mercury.

To secure the best results certain precautions are necessary: (1) if the internal diameter of the tube is much less than 3 mm. the parts of the mercury column will not be easily united when accidentally separated; if much greater than 3 mm. a good meniscus will not be formed in the horizontal part of the tube; (2) the part of the tube containing the confined air must have a uniform section; (3) the parts of the tube and the amount of confined air must be properly proportioned; (4) proper correction must be made for the irregularity due to sealing the end of the tube. This correction may be made in the following manner: let the length of the air-column measured from some fixed point near the closed end of the tube be l; then $l + x$ is the equivalent length in which x is the desired correction. Now take some other value of the length when

the pressure is different, and we shall have for the equivalent length $l' + x$. If the corresponding pressures are $H + h$ and $H - h'$, we have, assuming Boyle's law to be true,

$$(l + x)(H + h) = (l' + x)(H - h').$$

If this equation is solved for x, using known values of l and h we shall know how far the true zero-point for measuring volumes differs from the assumed zero-point. Taking the least and greatest values for l and the corresponding values of h from the preceding table and solving for x we find that $x = -.09$ cm. This means that the assumed zero-point for measuring volumes is .09 cm. on the negative side of the true zero-point.

For regular laboratory work the tube should be furnished with a suitable mounting. For example, an L-shaped frame may be used as illustrated by Fig. 4. The closed branch of the tube may be graduated,

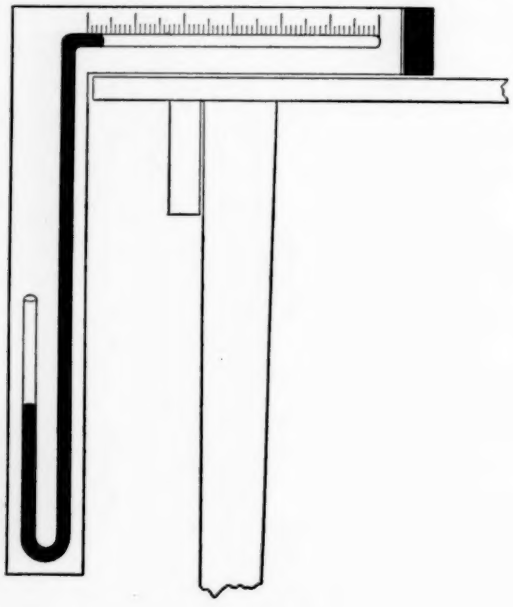


Fig. 4.

or a scale may be attached to the frame with its zero-point properly adjusted by trial as above described. The end of the frame at the closed end of the tube should be heavily weighted for the sake of stability.

THE M. K. S. ABSOLUTE SYSTEM OF UNITS.

By F. J. Rogers.

THE C. G. S. units of force and work are extremely small, so small that any ordinary problem in mechanics will require for its solution the use of enormously large numbers. For example, the force of gravity acting on 50 Kg. is 49,000,000 dynes; the work required to lift 100 kg. a height of 100 m. is 980,000,000,000 ergs; one horse-power-hour is equal to 26,800,000,000,000 ergs. Of course, these large numbers may readily be expressed by using powers of 10, as for example, one-horse-power-hour $= 268 \times 10^{11}$ ergs. But there are objections to this. Students who are just beginning to use the C. G. S. units generally have little facility in the use of powers of ten in numerical calculations. A more serious objection is the fact that the units used are almost imperceptibly small while the numbers which express the actual quantities dealt with are almost inconceivably large.

These difficulties have led me to make use of a system of mechanical units based on the meter, the kilogram and the second, or briefly the M. K. S. System. In this system the unit of force is that force which, acting on one kilogram for one second, will give it a velocity of one meter per second. This unit of force I have called a *large Dyne* or merely *Dyne*, spelled with a capital letter to distinguish from the C. G. S. dyne. The relation between these two units of force may be expressed as follows:

$$1 \text{ Dyne} = 1 \text{ Kg.} \frac{\text{M}}{\text{sec}^2} = 1000 \times 100 \text{ g} \frac{\text{cm}}{\text{sec}^2} = 100{,}000 \text{ dynes.}$$

In the M. K. S. system the unit of work or energy is the work done when a force of one Dyne moves its point of application one meter. This unit is equal to ten million ergs and is, therefore, exactly equal to one Joule. It follows that the M. K. S. unit of power is equal to one Watt.

The use of this system requires the introduction of only one new unit, namely, the large Dyne. Even this unit may advantageously replace the megadyne which is occasionally used.

In addition to the greater convenience in solving problems in mechanics, the M. K. S. system has the advantage of bringing the Joule and Watt into a harmonious mechanical system involving the meter and the kilogram.

I have used this system of units in a first year university class in physics during the past year with little danger of confusion with the C. G. S. system which was also used.

Of course, no system will be really understood unless a large number of problems are solved, and, moreover, problems of some degree of complexity.

A MODIFICATION OF MANCE'S METHOD OF DETERMINING BATTERY RESISTANCE.

By Louis W. Austin.

ONE of the most elegant methods of determining the resistance of a battery cell is undoubtedly the Wheatstone's bridge method of Mance. As ordinarily used, however, it labors under certain disadvantages. Leaving out of account the annoyance of adjusting the galvanometer zero by means of the control magnet, the main difficulty in the use of the method lies in the fact that at each observation the battery circuit must be kept closed long enough for the galvanometer to come to rest, which in the case of open circuit batteries gives ample time for the errors due to polarization to appear unless large resistances are inserted in the bridge, thus reducing the sensibility of the method and rendering impossible the use of the convenient slide wire bridge for such cells. The placing of a condenser in series with the galvanometer as suggested by Professor Lodge does away with the first and smaller difficulty, but does not help the second. The substitution of a low resistance telephone in place of the galvanometer very largely obviates both. It possesses the advantages that it responds only to the changes of the current, and is always ready for an observation; hence no delay is necessary after the battery circuit is closed.

To carry out this modified method the slide wire bridge is connected as in the ordinary Mance's method, the telephone being connected to the ends of the bridge, and an empty wire across from the point between the two resistances to the bridge wire. No sliding contact piece is used, but contact is made by touching the slide wire with the empty wire with a stroking touch—a fine wire gives the best sound. A key is placed in circuit with the cell which is closed only at the instant that the connection on the bridge wire is closed. The sharp click of the closing of the key differs so much from the rustling noise of the other contact that there is no trouble in distinguishing the one from the other. This closing of the battery circuit in a series of almost instantaneous contacts gives little chance for polarization, and the resistances of even the most rapidly polarizing cells in common use may be found with a considerable degree of accuracy. For approximate determinations the method is very rapid, the resistance being obtained with an accuracy within a few per cent. almost instantly, and with a little care the determinations can be made practically as exact and satisfactory as with the more complicated Kohlrausch alternating current method.

The University of Wisconsin, April, 1900.

NEW BOOKS.

Physics: Experimental and Theoretical. Vol. I., *Mechanics, Hydrostatics, Pneumatics, Heat, Acoustics.* By R. H. JUDE, D.Sc., M.A., Lecturer in Physics, Rutherford College, New Castle on Tyne. London, Chapman & Hall, 1899. Pp. xxiv + 926.

It is difficult to specify the character of this volume in any brief terms. In size, it almost equals the great compendious text-books so common in France and Germany, but as yet, for some reason, almost without existence in English. If the second volume is to be as large as Volume I., the work will exceed in bulk any modern general text-book of physics in our language.

Upon opening Dr. Jude's book, one is struck at first sight by the unusual allotment of space to the various topics. One is not so much surprised at the lack of space given to sound, for it is quite in vogue among modern writers to curtail this part of physics, as at the relatively large amount of space accorded to heat, as compared with mechanics. Looking more in detail at the selection of subjects for discussion, many other disparities meet the eye. The entire subject of capillarity, for example, is given within the limits of a single page, while one may easily select from neighboring portions of the book paragraph after paragraph given to unimportant and comparatively irrelevant matters, such as life belts, safety whistles, ball cocks, and the like. These might well have been sacrificed to the fuller treatment of the more vital and interesting phenomena which arise from the properties of the surface film of liquids. One might point out many other instances of the same kind. It is indeed a peculiarity of the work that a vast number of isolated topics receive brief and inadequate consideration. Some of these are of considerable interest in that they serve to illustrate some principle of physics, but the application is not always clearly brought out.

Among the faults of omission are the lack of an index, the almost complete absence of references to original sources, omissions which impair the usefulness of the volume considered as a book of reference (and it is too bulky to be conveniently used in any other way), and failure to indicate fully the sources from which the numerous tables of physics constants have been compiled. The value of these tables would have been greatly increased had the origin of the data been in all cases carefully given. The volume contains a very large number of miscellaneous ex-

amples and exercises, many of which are very good, while some are of doubtful utility. In these the metric system and the British systems of weights and measures are hopelessly mixed. For this, considering the deplorable example set by boards of science examiners throughout Great Britain, we ought perhaps not to hold the author wholly responsible, but we must hold him responsible for the introduction of new and questionable names for various derived units. One can but protest against such names as *velo* for a unit of velocity, *celo* for the unit of acceleration, *pul* for the unit of momentum, and above all against the use of *torque* for the unit of moment. Torque is already in universal use among physicists and engineers for the moment of a force tending to produce rotation.

The author attempts to justify himself in this matter in his preface, on the ground that it is "tolerably certain that much of the vagueness attaching in student's minds to sundry physical conceptions, arises from their units wandering about in an unbaptized state * * * ." As to the advantage of increasing the list of names assigned to science units, even when the nomenclature has been placed in the hands of committees of men best qualified for such work, we should differ fundamentally from Dr. Jude. When it comes to the free giving of names to such units on the part of individual authors, we desire to record vigorous disapproval.

In a book containing so many details, errors of all sorts are pretty sure to creep in. The vigilance necessary to exclude them all is something well-nigh superhuman. A British author could perhaps scarcely be expected to know that the Falls of Niagara which are, as he states, about 160 feet high, and may be assumed to deliver a certain amount of water per minute, do not strike Lake Ontario at all, since the base of the falls is distant several miles from the head of the lake; but when it comes to the scale of the hydrometer, as given on p. 129, one might expect the author to know that such a scale is not one of equal parts, and to demand of him that it should be drawn in accordance with the laws of the instrument.

It would be a thankless task to go through the volume, and point out the petty errors which meet the eye of the reader. Most of these will doubtless be corrected in later editions. I refer to such matters as the substitution of Döppler instead of Doppler, a slip which seems to be well-nigh universal among English writers, the statement that lampblack is a "perfect absorber," and the like; but there are certain deeper-lying faults in this volume which it is difficult to characterize and the existence of which makes the reading of the book, in the main, unsatisfactory. These we fear are not likely to be eliminated. They are those which arise, in all probability, from a failure on the part of the author to thoroughly assimilate and master all the minutiæ of the vast amount of information which he has attempted to get inside the covers of his work.

Such power over the details of a broad science like physics is rare indeed, and few, therefore, are the men whom one would like to see in charge of the task of writing a compendious treatise.

The style of Dr. Jude's book is vigorous, but not always dignified. One catches now and again a strident jarring overtone which reminds one too strongly of the recitation room; the insistent tone with which too often an overwrought teacher strives to impress the principles of his subject upon a listless class. It may perhaps be said of the treatise, with justice, that it is too obviously didactic in style; that it savors too strongly of the class-room and not sufficiently of the laboratory. It ought, in justice likewise to be said that there is a great deal of important material in the book, not always, alas, to be quickly found when wanted, and that some of the chapters on heat in particular are of very great interest and value.

E. L. N.

Physical Chemistry for Beginners. By Dr. CH. VAN DEVENTER. With an Introduction by Prof. J. H. VAN'T HOFF. Authorized American Edition from the German Edition. Translated by BERTRAM B. BOLTWOOD, Ph.D. New York, John Wiley & Sons; London, Chapman & Hall, Limited., 1899.

The Arithmetic of Chemistry. Being a Simple Treatment of the Subject of Chemical Calculations. By JOHN WADDELL, B.Sc., Ph.D., D.Sc. New York, The Macmillan Co., 1899.

The Spirit of Organic Chemistry. An Introduction to the Current Literature of the Subject. By Prof. ARTHUR LACHMAN. With an Introduction by Prof. PAUL C. FREER. New York, The Macmillan Co., 1899.

In 1893 there appeared in Amsterdam a little book on *Algemeene Scheikunde* (General Chemistry), for the use of students beginning the study of chemistry. The book was written in van't Hoff's laboratory; and van't Hoff used it in his classes as a text for that side of his course which dealt with general chemical principles and the physical aspect of chemical changes. It forms a very satisfactory, brief, introductory treatise on physical chemistry. Shortly after its appearance in Dutch it was reproduced in a German edition; and now, very properly, we have an English version.

The book considers, first, the conservation of mass, the laws of constant, multiple, and combining proportions, and Gay-Lussac's volume-combination law; then the properties of gases, and the ideas of molecular weight and of valence; and, next, the simpler facts of thermochemistry

and the current conception of dissociation phenomena and mass-action. All this is followed by a brief outline of the theory of solutions, a few pages on chemical optics, and, finally, a chapter on the periodic classification of the chemical elements. The author's treatment is clear and good; and he presents only matters which form an essential part of any broadly conceived introductory course on general chemistry.

Our next book, Waddell's "Arithmetic of Chemistry," is a collection of chemical problems, with careful explanations of the methods used in solving them. The principles applied are the stoichiometric laws and the theorems of Boyle and of Gay-Lussac (Charles). The author says: "The form that this book has taken is due to a very considerable experience of the difficulties encountered by students when attempting to make chemical calculations. The effort has been made to smooth away these difficulties, and to give an accurate, as well as a simple and systematic, treatment of the subject." Like the foregoing book, this one is intended as an aid in elementary chemical instruction. An appendix presents some miscellaneous matter, such as an account of the metric system, a comparison of thermometric scales, a table of combining weights, and a table of logarithms.

The final book under review, Lachman's "Spirit of Organic Chemistry," is a gem in its way. It is an attempt to give to the advanced student of chemistry a vivid picture of the historical development of a number (nine) of important lines of work in organic chemistry. It is thus designed to serve as a supplement to text-books on organic chemistry. That such a book must be a great help to the earnest student is obvious to anyone; the need for it is made clear by Professor Freer, in his brilliant Introduction, in the following words:

"The magnificent science of modern Organic Chemistry, with its countless compounds, * * * fascinates with its possibilities, while it appalls with its vastness. The student stands discouraged at the very threshold. How can he ever hope to master the general classification, let alone the minor details, which must become a part of his very being, if he too wishes to do his share, however small, toward completing and rounding out the still unfinished structure? The answer is plain: he can do this only by comprehending the *spirit of the science*, by learning its great theories, not as mere mnemonic efforts, but as the result of a development for which many of the most earnest and acute minds known to the history of science have fought and toiled."

J. E. TREVOR.

Outlines of Industrial Chemistry. By F. H. THORP. 8vo, pp. xx + 541. New York, The Macmillan Company, 1899.

In the preface of this book the author states that it is intended "to

furnish an elementary course in industrial chemistry" and that it is for the use of students who are at least "familiar with the elements of general chemistry, both inorganic and organic, and with the elements of physics." That a second edition was called for in a year after the first issue is sufficient evidence that a book of this kind is needed, and that it has been useful to students and those engaged in industrial chemical work.

The book opens with a very useful description of the more common manipulations in technical chemical work. An account follows of fuels and their preparation, especially coke and gaseous products; sulphur and its oxides and especially sulphuric acid are fully treated; no mention is made, however, under fuming acid of the use of platinized asbestos for making the anhydride with sulphur dioxide and air under pressure, the product being then absorbed by strongest oil of vitriol. Lunge gives a full description of this process and states that it is used in several factories in Europe.

Following the soda industries, concerning which the author states that the Leblanc process which was first used a century ago "still produces about half of the world's supply," the chlorine industry is amply treated; after describing the two most common methods in practice with and without the use of manganese binoxide, he describes some of the electrolytic processes for producing chlorine and caustic soda, and concludes with the opinion that they are not yet so perfected as to lead manufacturers to invest in them.

For getting the ammonia from the gas liquors Feldman's apparatus is the one most used in this country; a part of the heat for the distillation of the ammonia is produced by the combination of the base with sulphuric acid to produce the sulphate; some of the ammonia used in this country is made by the dry distillation of waste animal matter from slaughter houses and tanneries.

Under fertilizers the statement that the majority of the artificial fertilizers contain only one or two of the three plant foods that go to make a complete mixture is hardly correct; an examination of the lists of analyses made at the various experiment stations shows that by far the larger number of these mixtures contain all three of the plant foods, nitrogen, potash and phosphoric acid. In most of the experiment stations the same value is allowed for the reverted as for the water-soluble phosphate; therefore the other statements that the phosphate fertilizer is usually valued according to its percentage of water-soluble phosphate, and that reversion of the soluble to the reverted condition is a serious matter for the manufacturer and the buyer are not quite right.

The preparation of lime, cements, ceramics, pigments and a number of chemicals in use on a large scale occupies a large part of the book, as

does also the treatment of the textile industries. Under illuminating gas the so-called water gas, consisting largely of carbon monoxid and the hydrocarbons necessary for the luminosity, is mentioned as having replaced coal gas very extensively, over which it has many advantages, such as greater illuminating power, a smaller plant, and less labor; the one serious objection due to its high percentage of carbon monoxide, and consequent exceedingly poisonous character, cannot be avoided.

Under fermentations Hansen's eight typical species of yeasts are briefly described, and the advantages gained in using only pure yeasts, especially in the manufacture of beer, are particularly emphasized. The process of pneumatic malting is claimed to insure a good product in any season; a constant stream of moist air, kept at a temperature of 14° C., passes through the grain in a wire gauze drum.

Under textile industries the several kinds of fiber used, and the processes of bleaching and mordanting are described; a long list of coloring matters is given, and the methods of dyeing and textile printing are very fully explained.

An interesting account of the manufacture of wood pulp paper is given under the paper industries. Explosives, and especially those with nitroglycerine as a basis, receive special treatment.

It would take too much space to enumerate all the industries described. All the important manufactures in which chemistry is concerned are included in the list. The author confines himself to methods and processes that are in actual use or in which there is some prospect of usefulness, and practically no space is wasted in the discussion of unsatisfactory processes. The print is clear, the illustrations though plain are well defined, and the descriptions are as complete as could be expected in consideration of the number of topics treated.

G. C. CALDWELL.

Central Station Electricity Supply. GAY AND YEAMAN. London, Whittaker & Co., New York.

This book is primarily written for those connected with the design and operation of central stations, and it furnishes an excellent basis for a general knowledge of these subjects. The machines and other apparatus described are mainly of English manufacture, but the sections which discuss engineering practice show this to be similar to that of the United States in many particulars. The work should be appreciated by all engineers who have any connection with the supplying of electrical energy.

The authors of the work are practical men in charge of electric lighting interests, and the text gives the essentials of this particular branch of engineering. The title of the work would have been more complete

had it included the statement that the electricity supply was for electric lighting purposes. The hints and suggestions as to the best modes of operating the several parts of a station equipment for best efficiency are very helpful. The authors have wisely adopted the practice of summarizing the data in regard to particular pieces of apparatus so that their various features may be seen at a glance. In their desire to make the work complete they have occasionally included descriptions of commercially unsuccessful apparatus in such a manner that an inexperienced reader might have difficulty in distinguishing it from that in common use. As a rule, however, they discriminate between the really useful devices and those which merely look well upon paper.

The plan of the work is systematic and progressive. After a brief summary of the legal and business history of electric lighting and a description of the experiments leading up to its commercial application, the location and general arrangement of the building are discussed. The steam plant and the electrical equipment are given detailed treatment in a manner which shows intimate knowledge of the requirements of the problem. The book brings out clearly the extent to which the accumulator or storage battery forms a part of the modern central station and thus shows that this useful device for increasing station economy is well appreciated in England. In connection with the steam and electrical plants all of the modern labor-saving appliances are described, and while no distinct recommendations as to the choice of particular pieces of apparatus are made, the advantages and disadvantages of each are clearly set forth. With these data the engineer can make an intelligent selection for a particular case. Some attention is also given to the distribution of the electrical energy after its generation and a number of appendices give practical matter which does not properly belong to the body of the work. The book, which forms one of the well-known and excellent "Specialist's Series," may be read with profit by both engineer and student.

HENRY H. NORRIS.

Text-book on Physics. Sound. By POYNTING AND THOMSON. London, Chas. Griffith & Co., 1899. Pp. x + 163.

This, which was announced as the second of a series of four volumes constituting the text-book of Professors Poynting and J. J. Thomson, is the first to appear. The first volume of the series, on *the properties of matter*, is promised shortly. The remaining volumes are to treat of *heat*, *magnetism*, *electricity* and *light*.

There is greater need, perhaps, at the present moment of a concise treatise on the subject of *sound*, not too difficult for the beginner and at

the same time sufficiently rigorous in its treatment, than in any of the other parts of physics. This the authors have succeeded in giving us. The volume contains just enough of the physiological and the musical elements of the subject to relieve it from the dryness and severity almost unavoidable in treatises on wave motion, and at the same time it deals at sufficient length with the analysis of vibrations to give the student a definite idea of the manner in which curves are built up by the superposition of harmonics and of the application of Fourier's theorem to the determination of their components.

That the work in spite of its brevity (163 pages), is not without a certain completeness will appear from the following enumeration of the points covered in discussing the experimental determinations of the velocity of sound. We find in the few pages devoted to this topic a statement of the corrections for moisture and temperature, a brief but entirely definite and intelligible account of Derham's experiments on the effect of wind, a discussion of the method of reciprocal observations, in which it is shown that the velocity is given with a sufficient degree of approximation by dividing the distance between the stations by the mean of the observed intervals. Then follows a statement of the results of the French Academy's observations in 1738, of the experiments of 1822, of the long-distance determinations made by Moll and Van Beck in 1823 near Utrecht, of the Tyrolese and Swiss experiments at different levels, of the measurements of the velocity of sound in the Arctic regions, of Stone's determinations at Capetown in 1871, and of Regnault's work in which sound waves were made to record their arrival electrically. The section closes with an account of Jacques's experiment on the influence of intensity on the velocity of sound, a description of the extraordinary observations made upon the sound waves from the Krakatoa eruption, and a brief statement concerning the velocity of sound in water and in isotropic solids.

All of this material is given in the space of eight pages. The reader who desires simply the final data will find what he needs, while he who wishes to read the full accounts of these various experiments, will obtain all the necessary references in the footnotes. We cite this as an example of the manner in which Professors Poynting and Thomson have worked out the physics of their subject. It is to be hoped that the remaining volumes of the series will exhibit the same remarkable combination of clearness, completeness and consciseness, the same good judgment in the selection of material and the same accuracy of statement.

We note the substitution, usual in English text-books, of *Döppler* for *Doppler*, and the not uncommon omission of the final s in *Lissajous*. Where the name of the latter observer is used in the possessive case, only the most strenuous effort on the part of author and proofreader will prevent printers from placing the apostrophe before the finals. E. L. N.

Magnetic Induction in Iron and Other Metals. By J. A. EWING. Third edition revised. Pp. xviii + 393. New York, D. Van Nostrand Co., 1900.

This is the third edition of Prof. Ewing's well-known treatise. It contains in addition to the subject matter of former editions a new chapter on practical magnetic testing, in which the author describes the permeability bridges of Eickemeyer, Kennelly, and Holden, the yoke apparatus of Koepel and Kath, Brüger's apparatus for testing the induction in the air gap by means of the bismuth coil, and du Bois' magnetic balance. He also gives a brief account of some of his own more recent apparatus; such as the Ewing magnetic balance, and the Ewing hysteresis tester. With the exception of the very brief reference to the use of bismuth in magnetic measurements just alluded to, no account is given of the very important and interesting researches of Lenard and others upon the properties of this metal in the magnetic field. No mention is made of the very beautiful method recently developed by Braun in which the deflection of the kathode rays is utilized for the studying of alternating magnetic fields; nor of the attempts made by Maurain and by Wien to separate losses of energy due to true hysteresis from those resulting from the action of Foucault currents. Indeed, with the exception of the description of the practical apparatus mentioned above, the present edition is essentially a reprint of those which have gone before, with a few additional references in the footnotes to investigations which have appeared since the date of the previous edition, and a few new articles such as those describing the effects of temperature as determined by Dr. Morris, p. 190, and Partridge's study on the aging of iron by prolonged exposure to a moderate temperature, p. 193.

E. L. N.

Oswald's Klassiker der Exakten Wissenschaften, No. 109. Pp. 121. Leipzig, Wilhelm Englemann, 1899.

This number of Ostwald's series contains a translation of Felici's three memoirs on the mathematical theory of electro-dynamic induction by Dr. Dessau of Bologna. These papers, which appeared in the original in the annals of the Tuscan University, Pisa, 1854 and 1855, and are in that form inaccessible to many readers, are of no little importance in the development of our knowledge of induction. Felici was the first to test experimentally the laws of voltaic induction, and throughout his work experiment went hand in hand with theory. He made it his object not only to determine as far as possible the causes of induction, but likewise to give an exact mathematical formulation of the laws thus experimentally established.

In translating these important memoirs into German, and thus bringing them conveniently to the hand of physicists in general, Dr. Dessau has placed that portion of the scientific world which interests itself in mathematical theory under great obligations.

At the end of the volume, there is a brief but appreciative sketch of Felicic's life, and a series of useful notes by the editor. E. L. N.

The Theory of Electrolytic Dissociation and Some of its Applications. By HARRY C. JONES. Pp. vi + 289. New York, The Macmillan Co., 1900.

After an introductory chapter on "The Earlier Physical Chemistry" (pp. 1–70), the remaining three chapters of this volume are devoted to "The Origin of the Theory of Electrolytic Dissociation" (pp. 71–103), "Evidence Bearing upon the Theory of Electrolytic Dissociation" (pp. 104–170), and "Some of the Applications of the Theory of Electrolytic Dissociation." The contents of the book are fairly indicated by these headings.

The first chapter is quite in place, though, in the reviewer's opinion, it might with advantage have been somewhat abridged as regards numerical data. The second and third chapters give an elementary account of the subjects treated. They do not show any depth of research and might have been computed almost entirely by the mere aid of Ostwald's "Lehrbuch." Nevertheless, they are remarkably easy reading, and serve their purpose, which is more or less qualitative, very well.

The last chapter is less satisfactory. Even in an elementary book, the author should be careful about logical accuracy and be sure that, however much or little he may *say*, he *knows* everything of importance that has been said on the subject. The reviewer's impression from this chapter is that the author has not worked sufficiently on the original memoirs, and has not so absorbed the subject as to be able to present it in a clear and complete form which is his own and not Ostwald's or Nernst's. There is sometimes a lack of definiteness in the statement of problems, and insufficient stress on the more difficult points which are sure to occur to the keen student.

A more unpleasant defect, to the reviewer's taste at least, lies in the fact that the English of the book, though perfectly clear, is often faulty from a grammatical point of view. It may be useless to expect a proper use of shall and will, or of should and would, and it may be carping to object to split infinitives; but we wish that the author had paid more attention to his moods and tenses in conditional sentences.

But after the disagreeable task of finding fault, it is pleasanter to return to the good points. The student will not be injured seriously by a

few slips; for if the subject is of any importance to him, he will certainly go on, and then upon working it through for himself he will easily correct what few false impressions he has received. Dr. Jones's book will hardly be indispensable to anybody, but it forms an easy and agreeable introduction to the study of Electrolytic Dissociation, and we think it will prove useful to many chemists who are beginning the subject.

EDGAR BUCKINGHAM.